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Of the Prefident's Effay on the Variations of the Atmofphere.

E R R A T A.

Page 304, In Table the 2d, in the line of Reaumur's Thermometer, *for* 10°. 9°. 8°. 7°. 6° 5°. 4°. 3° 2°. 1°. *read them*—10°—9°—8°—7°—6°—5°—4°—3°—2°—1°.

—— 305, —— 5, from top, *for* are determined, *read* be determined.

—— 310, —— 10, from do *for* complement of 9°,5, *read* complement of—9°,5.

—— 311, —— 1, from do. *for* complement of 9°,5, *read* complement of—9°,5.

—— 317, —— 6, from do. *for* 1799, *read* 1790.

—————— 8, from do. *dele the point after* foil *and add* a comma.

—— 319, —— 3, from bottom, *for* 582,175 grs. *read* 582175 grs.

—— 339, In the ſixth column, and *under* the Expanſions of one Inch at 32°, line 8 from top, *for* ,1250909, *read* ,1250900; and line 9, ſame column, *for* ,1295557, *read* ,1295575.

—— 340, —— 6, from top, *for* D'Amontoms, *read* D'Amontons.

—— 344, —— 5, from do. *for* Tremly, *read* Trembley.

—— 345, —— 6, from bottom, *for* $\frac{1}{4}$, *read* $\frac{1}{34}$.

—— 365, In the fourth column, and 2d line from top, *under* above by obſervation, *for* 5°, *read* 45°

—— 373, —— 6, from top, *for* mole, *read* Mole.

—— 407, —— 1, at ditto, *for* Sect. VI. *read* Sect. V.

—— 409, In the fourth column, and 1ſt line, *under* Mathem. *for* 80,7°, *read* 80°,7.

—— 413, —— 3, from top, *for* of the hemiſphere, *read* as the northern hemiſphere.

—— 417, —— 1, at top, *for* Sect. VII. *read* Sect. VI.

—— 418, —— 5, from top of the note, *add* after beyond which, the increaſe of.

—————— 8, ditto of note, *for* itt maximum, *read* its maximum.

—— 429, —— 2, from bottom, *for* 328, *read* 32°.

—— 441, —— 6, from ditto, *for* manomeeers, *read* manometers.

—— 446, —— 5, from top, *for* 2,44 × 36, *read* 2,43 × 36.

—— 491, —— 7, from ditto, *for* hitherto, *read* heretofore.

—— 493, —— 14, from top, *for* neceſſaty, *read* neceſſary.

—— 500, —— 1, at ditto, for Hydrometer, read Hygrometer.

Of the VARIATIONS *of the* ATMOSPHERE.

By RICHARD KIRWAN, *Esq; L.L.D. F.R.S. and P.R.I.A.*

THE variations of the known properties of the atmofphere, viz. of its ftate of moifture or drynefs, and its difpofition to admit, retain, or difmifs the watery fluid, its temperature, gravity, elafticity, and electricity in different feafons, and frequently in the fame feafon, and in different years, in the fame or in different latitudes, at the fame or at different diftances, whether vertical or lateral, from the oceans feparating the two continents that form the folid mafs of the globe, as well as on thofe oceans themfelves, and the currents which from different points with more or lefs permanency agitate the whole atmofpheric mafs, form, together with the inveftigation of their caufes, the principal objects of meteorological fcience. Read May 11, 1801.

THOUGH this ſcience has been cultivated with much ardour in many parts of Europe ever ſince the diſcovery of the gravity of the fluid that ſurrounds us, it muſt be owned very little progreſs has hitherto been made in it. In every other branch of phyſical ſcience from a knowledge of the cauſes we are enabled to foreſee the effects; but from a knowledge of the actual ſtate of the above mentioned properties of the atmoſphere, who can with certainty foreſee what it ſhall be the next day?

THE obſtacles which originally obſtructed all advances in this ſcience were,

1°. THE want of proper inſtruments to meaſure the proportion or quantities, whether of moiſture in the atmoſphere, or of its weight, or its temperature, its denſity or its electricity.

2°. THE want of general ſtandards to which, as to their proper units, the ſeveral degrees of each property may be referred.

3°. INTERRUPTION of the obſervations of the ſtates of the atmoſphere, by reaſon of which their connexion muſt remain unknown.

4°. As the actual ſtates of the atmoſphere are intimately connected, not only with the preceding, but with the contemporary ſtates in different and moſt diſtant parts at leaſt of the ſame hemiſphere, the neglect of comparing either of them, as far as

they

they were or might be known, has been, and is perhaps at present, the principal source of our ignorance of the causes that produce, vary, or interrupt them. Most meteorologists content themselves with noting the diurnal state of the atmosphere in the place of their residence, without any referenee to its state in distant countries; and thus, for want of concert, *dum singuli pugnant omnes vincuntur*.

5°. The neglect of observing the superior currents of the atmosphere, which may at present be detected by tracing the direction of baloons, in all seasons.

6°. The neglect of maritime observations, which, being freest from all local influences, are by far the most instructive, and should, if possible, be made in all degrees of longitude and latitude.

However, many of these obstacles have of late been happily removed. The proportion of moisture in the atmosphere relatively to its saturability, in its various states, whether of density or temperature, can now be perfectly ascertained by the hygrometer of the late excellent Mr. De Saussure, who has by the most extraordinary efforts of genius and labour improved that instrument, so as to make it speak a language every where uniform and intelligible; a degree of perfection ever before thought unattainable. Nay, by a number of most ingenious and delicate

experiments, he has been enabled to make it indicate very nearly the abſolute quantities of moiſture contained in given cubic meaſures of atmoſpheric air of given denſities and temperatures. Of this inſtrument and its uſes, as Mr. Sauſſure's treatiſe has never been tranſlated into our language, and ſeems unknown to our meteorologiſts, I ſhall give an abridged account in the following pages.

Mr. De Luc has alſo beſtowed much laudable induſtry in perfecting an inſtrument, applicable to the ſame purpoſes, but leſs fragile; how far he has ſucceeded I am not enabled to ſay, not being poſſeſſed of it.

Barometers have been brought to the utmoſt degree of perfection by the ſagacity and unremitting labours of Mr. De Luc, as may be ſeen in his immortal work on the modifications of the atmoſphere. The varying denſities of the atmoſphere at different heights (denied by Buffon) have been ſatisfactorily proved by the numerous and happily contrived experiments of Meſſrs. Sauſſure the father and ſon. See 36 Roz. p. 98.

Thermometers have long ſince been improved by Fahrenheit and Reaumur; but the imperfections and uncertainties attending many under this laſt denomination have been detected by Mr. De Luc, who in their room introduced a much more perfect inſtrument

ment which ought to bear his name. Yet the inconveniences attending the variations of the fcale have not been fufficiently felt by fome fubfequent philofophers, who thus unhappily, and to no purpofe, multiply the labours of the meteorologift. Mr. Van Swinden's excellent treatife on thermometers enables us to underftand the difcordant languages of many of them. And of late centefimal thermometers (the fame as the Swedifh) have been introduced in France. The degree of cold at which mercury freezes was determined by Mr. Cavendifh from the experiments of Mr. Hutchins.

Electrometers of the leaft fallacious kind have been devifed by Mr. Cavallo.

Of *Anemometers*, though many have been invented to denote the direction and the force of winds, yet none of them has, as yet, been introduced into general ufe. Mr. Smeaton's table of the rapidity and force of the winds, Phil. Tranfactions 1759, feems the beft that has as yet appeared.

2° With refpect to *ftandards*, Sir George Schuckburg has fhewn, from 132 obfervations made in Italy and in England, that the mean height of the barometer at the level of the fea, the temperature of the mercury being 55° and of the air 62°, is 30,04 inches,

inches, Phil. Trans. 1777, p. 586. We may then aſſume the height of 30 inches as the natural mean height of the barometer at the level of the ſea in moſt temperatures between 32° and 82°; for if the mercury were cooled down to 32°, that is 23° below 55°, it would be lowered by that condenſation only 0,07 of an inch; and if it were heated up to 80°, that is 25° above 55°, it would be raiſed only ,078 of an inch; quantities which except in levelling may be ſafely diſregarded.

The French have heretofore conſidered 28 Paris inches as the mean height of the barometer at the level of the ſea, that is 29,84 Engliſh inches. But from 1400 obſervations made at Rochelle by Fleaurieau De Bellvue, and from five years obſervations made at Port Louis in the Iſle of France, he concludes the mean height of the barometer at the level of the ſea to be 28 inches and 2 lines and $\frac{10}{12}$ of a line in the temperatures of from 52° to 55° Fahr. = 30,08 Engliſh inches, 4 New Roz. p. 159*. Hence we may conſider in round numbers 30 inches as the ſtandard height of barometers at the level of the ſea. And knowing the true height of any part of the earth, we may by ſubtracting that height expreſſed in fathoms from the log. of 30, viz. 4771,21, find the logarithm which indicates the number of inches at which, as

* La Chappe thought it 28 inches 1,5 lines. See Beguelin's Memoir, Mem. Berl. 1769. 12 Coll. Acad. 424.

as its natural mean, the mercury ſhould ſtand at that height over the level of the ſea.

Thus ſuppoſing the height to be 87 feet, equal 14,50 fathom, then 4771,21—14,50 = 4756,71, which is the logarithm of 29,9; this therefore is the natural mean height of the barometer at the elevation of 87 feet over the level of the ſea.

Consequently to all the heights heretofore, calculated by the French over the level of the ſea, 139,32 feet muſt be added, Engliſh meaſure, when the Mercurial height at the level of the ſea was barely ſuppoſed to be 28 French inches.

With reſpect to *temperatures*, I have eighteen years ago indicated a ſtandard (none having been before thought of) with which the temperature of all the inhabited parts of the globe, whoſe latitudes are given, may be compared. La Cotte, whoſe unceaſing labours contribute ſo largely to the progreſs of meteorological knowledge, has ſince extended this compariſon to 200 towns in 30 different degrees of latitude, and found their mean temperature to coincide with my eſtimation twelve times. That the greateſt error was 6,75 degrees of Fahr. which occurred four times, and the mean error to be 0,36 of a degree of Fahren. 37 Roz. p. 412. Such errors, not in the principles but in their application, might

might well be expected where the mean temperature of only a few years obfervations could be procured. On this occafion I cannot forbear remarking, that, in calculating the mean temperature of places on the obfervations of a few, or even of many years, the extremes, whether of heat or cold that happen not above once in ten years or ftill more rarely, fhould be omitted and only mentioned in a note.

THE third obftacle, viz. the interruption of obfervations, and alfo the 4th, 5th and 6th, cannot be remedied but by the eftablifhment of meteorological focieties in different parts of Europe, annually communicating with each other (fo liberally endowed as to be enabled to employ proper obfervers at proper diftances), and obliged to publifh annual accounts of their feveral obfervations, and to compare them with thofe made in foreign countries. When the vaft importance of meteorological knowledge to agriculture and navigation is confidered, there is at prefent fome room to hope that eftablifhments of this nature may take place at no very diftant period.

THE number of perfons, who, from a view of attaining a fettlement in fuch eftablifhments, would receive an education in the different fciences connected with meteorology, (that is indeed all the different branches of mathematics and natural knowledge), and

and the emulation, thus excited, would neceſſarily diffuſe a conſiderable maſs of knowledge throughout the community.

Theodore, the late Elector Palatine of the Rhine, realized this project in his dominions; and that incomparable ſovereign, *Catharine the Great*, entertained the ſame deſign ſhortly before her deceaſe.

Some other petty impediments ſtill remain, which unqueſtionably occaſion a great loſs of time, but which meteorologiſts themſelves may eaſily remove, namely, the diſcordant notation of their inſtruments, and the uſe of duodecimal, nay even in ſome caſes, of ſexageſimal diviſions, inſtead of the decimal, which is ſo much more commodious, and the application of the vague terms, *hot*, *cold*, *wet*, *dry*, to certain periods, without any deſignation of their preciſe meaning.

Among the moſt zealous promoters of meteorology in the laſt century we find ſome of the moſt eminent philoſophers it produced, Derham, Halley, De la Hire, Mairan, Reaumur, Wargentin, Leche, Kraft, Braun, Richman, Sauſſure, Beguelin, &c. and among thoſe now living, De Luc, Doctor Horſeley, Van Swinden, Cotte, Monge, &c.

The late great navigator Captain Cooke, and the ever to be lamented La Peyrouſe, Mr. Dalrymple, and Captain Phipps, have

 materially

materially benefited this ſcience by regiſtring the variations of their barometers and thermometers during their voyages.

Of the Moiſture of the Atmoſphere.

Moisture may be conſidered as riſing into the atmoſphere, or ſubſiſting in it, or deſcending from it. I ſhall firſt conſider its aſcent.

CHAP. I.

Of Evaporation.

Evaporation is the converſion of a liquid (and frequently even of a ſolid) into an inviſible fluid. That the liquid is diminiſhed is evident from its loſs of weight; but that the aqueous liquid, which I have here principally in view, is not decompoſed, is equally clear, ſince the inviſible fluid, into which it is converted, may be made to re-appear in its former viſible ſtate.

This converſion may be effected either naturally or artificially: that produced by nature is always accompanied by contact with the atmoſphere; the artificial is producible in contact with the atmoſphere, or even in *vacuo*, as is now well known.

In the common courſe of nature five cauſes concur in producing evaporation: heat, affinity to atmoſpheric air, agitation, electricity, and light.

Section

SECTION I.

Of the Influence of Heat.

THE connexion between heat and evaporation is too obvious to have efcaped the notice of even the moft inattentive to the operations of natural caufes. Every one knows that, in general, the greater the heat applied to water in fimilar circumftances, the more quickly it will be converted into vapour. As aqueous vapours are by far the moft abundant in the atmofphere, to their confideration I chiefly attend in this Effay. I have faid *in general*, becaufe Leidenfroft's experiments, and fome others which will be mentioned in the fequel, fhew that this law muft be circumfcribed within certain limits.

THAT all liquids are expanded by an increafe of heat is alfo well known. This rarefaction is oppofed by two powers, namely, the attraction of their integrant molecules to each other, and that of external preffure. But there is fcarce any degree of heat fo low, as to be totally difabled to overcome the refiftance oppofed by the power of integrant attraction; as appears by various experiments, in which, external compreffion being diminifhed or withdrawn, the rarefaction and evaporation could arife from heat fingly. Thus Sauffure found that water is more readily converted into

 vapour

vapour *in vacuo*, or in rarified air, than in air of greater denſity; becauſe, as he well remarks, all external preſſure reſiſts the expanſion of water into vapour, *Hygrom.* 219. It is true that he adds, in the ſubſequent page, that, in a given time, water loſes more of its weight by expoſure to the open air than it does *in vacuo*; but to prove this fact he only quotes the experiment mentioned in the 55th vol. of the Philoſophical Tranſactions, p. 146*, where indeed it is ſtated, that a cup of water ſtanding under an exhauſted receiver, loſt only 2,5 grains in 24 hours, whereas the ſame quantity of water expoſed to the open air during that period of time, loſt 35 grains. But it is plain that, even ſuppoſing the temperature and ſurfaces of the water in both cups equal, the reſiduary air under the receiver (whoſe capacity is not mentioned) muſt have been ſaturated long before that in the room could have been ſo, and thus its evaporation ſpeedily checked. It is expreſsly ſaid that the air under the receiver was only 43 times rarefied; enough therefore remained to be ſoon ſaturated: but if it had been ever ſo perfectly exhauſted, the elaſticity of the vapour already raiſed would have been ſufficient to check farther evaporation. Beſides, ſince it is acknowledged that water evaporates more eaſily *in vacuo* than in open air, it neceſſarily follows that, if the temperature remained the ſame, and if the vapours had liberty to eſcape, it would alſo evaporate more copiouſly. The quantities evaporated in ſmall portions of time are thoſe that ſhould be compared with each other;

* By miſtake he quotes the 59th vol.

other: this I have done, and found the loſs *in vacuo* incomparably greater than that in open air; the difference however I cannot now ſtate, having loſt the note I took of it. Mr. Watt has ſhewn, that water boils *in vacuo* at a degree of heat little ſuperior to that of the human body, and diſtills as faſt as it does in open air at 212°, 1 Mem. Mancheſter, 390. Water has alſo been diſtilled *in vacuo* by Wallerius, 11. Collect. Acad. 166, &c. and by the immortal Lavoiſier, Mem. Par. 1770. Hence in nature water evaporates more when the barometer is low than when it ſtands high, and conſequently diſtillation may be carried on on the tops of mountains more readily, and with leſs heat, than at the level of the ſea.

According to Mr. Cavendiſh, water heated to 72° is not (ſuddenly) converted into vapour under an exhauſted receiver, until the preſſure is no greater than about $\frac{1}{40}$ of the uſual preſſure of the aſmoſphere, that is about three-fourths of an inch; and if the water be heated only to 41, the preſſure muſt be reduced to one-half an inch, but the vapours thus raiſed are immediately turned back into water on reſtoring the preſſure of the atmoſphere, Phil. Tranſ. 1777, 622. But it is plain that he means, that the greater part only is re-converted into vapour (unleſs the air is already ſaturated) by the increaſed preſſure; for he allows that water expoſed to the open air will evaporate at any

any degree of heat, and under any preſſure; and this he attributes to the action of air upon it. And ſince the removal of the greater part of the preſſure promotes evaporation, the diminution of a ſmall part of the preſſure muſt promote it in a degree proportionably ſmaller, let the temperature be what it may. Hence vapours ariſe more copiouſly and readily in rarefied air than in air of greater denſity, if the temperature of both ſorts of air be equal.

The conſequence deduced from theſe experiments is farther ſtrengthened by the obſervations of Sauſſure on evaporation on the Col de Geant compared with that which took place at the ſame time at Geneva. On the mountain the barometer ſtood at 19,88 Engliſh inches, and at Geneva at 29,04. The thermometer on the mountain was at 45°,3 of Fahr. at a mean, and at Geneva at 50°, yet on the mountain 39,5 grains were evaporated, while at Geneva only 14,5 grains diſappeared. It is true that the air at Geneva wanted only 2,38 grains to ſaturate it, while that on the mountain wanted 4,03; but ſo ſmall a difference cannot account for ſo conſiderable a diſparity in the effect. See the ingenious calculations and the ſeries of conſequences deduced by this excellent philoſopher, *Voyages aux Alpes*, vol. vii. 8vo. § 2059 and 2060. His general concluſion is, that a diminution of the denſity of the atmoſphere amounting to one-third, every thing elſe being equal, doubles the quantity of evaporation, § 2062.

HENCE

Hence Mr. De Luc and ſome other philoſophers conclude, that the affinity of air to water, if any, no way contributes to evaporation, but that it is the mere reſult of the abſorption of caloric by water at its ſurface, which abſorption renders a portion of it ſpecifically lighter than the air in contact with it, *Idees ſur la Meteorol.* § 2, &c. The denſity, to which vapours thus raiſed are capable of arriving, hath certain *limits*, each relative to their temperature, being more extenſive in higher temperatures, and narrower in low temperatures. The ſpecific gravity of vapour he conſiders as leſs than that of air by more than one-half, their maſs being above one-half leſs than the maſs of an equal volume of air poſſeſſing the ſame elaſticity in the ſame circumſtances, ibid. § 7. Again, if, ſays he, theſe vapours ſubſiſt in any ſpace, unmixed with air, in that caſe any preſſure, ſuperior to the expanſive force which they poſſeſs in a given temperature, will, by forcing their particles into cloſer contact, oblige them to unite, and thus partially condenſe them, or (as he, in my opinion, improperly calls it) decompoſe them; but if any quantity of air, capable of ſupporting this exceſs of preſſure over their expanſive force, be mixed with them, they will ſupport their vaporous ſtate, § 14 and 554.

I muſt confeſs that the commonly received doctrine of the ſolution of vapours in air, at leaſt in air of the uſual denſity, on plains,

plains, appears to me much more intelligible and better founded. In this syſtem, firſt ſuggeſted by Halley, and beautifully illuſtrated by Roy, and ſtill farther extended by Doctor Hamilton, water is attracted by air, and forms a compound lighter than *mere* air of the ſame temperature, and under the ſame preſſure; but the difference of their gravities is not conſiderable. Whereas, in Mr. De Luc's hypotheſis, water and air, not being chemically united, do not form a compound, but rather what in technical language is called an aggregate, like water and oil mixed together; if ſo, vapours, being ſpecifically lighter by one-half than air of the ſame temperature, ſhould riſe above the ſtratum of air in which they are formed, until they are cooled during their aſcent, or meet with air of the ſame denſity, betwixt which and air of ſuperior denſity they muſt remain. If ſo, vapours ſhould never be found in the lower ſtrata of the atmoſphere, unleſs they be perpetually renewed; conſequently, if the ſource of evaporation be cut off, as it is when moiſt air is placed under a receiver ſtanding on metal, or on any other dry ſubſtance, and the temperature be kept undiminiſhed, the vapour originally diſperſed through the whole of its volume ſhould gradually collect and float at the top, as happens when water and oil are mixed together.

But this ſeparation of air and vapour never happens. For if concentrated ſulphuric acid, or dry cauſtic tartarin, or muriated lime,

lime, be, after ſome days, carefully introduced under the ſame receiver, all the moiſture will, after a few days, be extracted by them, let the receiver be ever ſo tall, as I myſelf and others have frequently experienced; which could not happen if theſe ſubſtances were not in contact with the vapours, which are perpetually drawn down by the attraction of the lower portions of air as faſt as they are deſiccated, and thus the contact perpetually kept up. Moreover, cold is known to be produced in air by evaporation, but cold is never produced by mixture, but it often is by ſolution. Many other arguments might be adduced in proof of the ſolution of vapours in air, but theſe appear to me ſufficiently convincing. See alſo Schmidt's excellent treatiſe on vapours in 4 Gren's Jour.

Yet it muſt be remarked, that though air favours the riſe of vapour by its affinity, yet it oppoſes its formation by its preſſure, and, conſequently, does not contribute to its origin, but only receives it in its naſcent ſtate, and diſperſes it through its own volume, and enables it to ſupport a degree of cold, greater than it could bear if ſingle; and as its preſſure is proportioned to its denſity, denſe air oppoſes evaporation more than rarefied air does, as already ſeen.

But denſe air requires a greater quantity of vapour to ſaturate it than rarefied air, and, therefore, in equal volumes it may contain

tain a greater quantity of moiſture than rarer air, and in all caſes air muſt have, to admit vapour, liberty of expanſion; for in veſſels full of air, and perfectly cloſed and reſiſting dilatation, no more vapour can riſe but as much as ſaturates that air; and, conſequently, diſtillation cannot take place, let ever ſo much heat be applied, as Fontana has ſhewn, 13 Roz. Journ. p. 22.

Doctor Eason, in 1 Mem. Mancheſt. p. 396, argues, that if vapours were diſſolved in air, the air that receives them would have its weight increaſed by the additional weight of the vapours; but it is known, that moiſt air is lighter than dry air. This is certainly true, but then it is only ſpecifically lighter than dry air. And it is well known that many, even metallic, alloys, are ſpecifically lighter than either of their ingredients. Air receiving vapours acquires a ſtill greater addition to its volume than it does to its weight; beſides, vapour is lighter than air, not indeed 1800 times, for that is true only of *ſteam*, (the vapour of boiling water) not of common vapour, as *De Luc* himſelf now allows.

The Doctor adds, that if the doctrine of ſolution were true, we could not have rain unleſs the air were ſuperſaturated with water, which is true with reſpect to ſome or other of the *ſtrata* of the atmoſphere, for rain proceeds from clouds, and clouds conſiſt of vapours

vapours which had paffed the limits of faturation, and therefore have, in fome degree, coalefced; but their farther coalefcence into rain is owing to electrical agency, as the Doctor himfelf well explains in the fucceeding pages.

THE evaporation of water expofed to the air is encreafed or diminifhed by various circumftances, fome relative to the water and fome relative to the air. Thofe relative to the water are, 1°. its temperature; 2°. its furface; and, 3°. its purity.

AND 1°. as to *temperature*. It appears from Lambert's experiments made, in a room in which there was a ftove whofe heat could be regulated, that evaporation proceeds in a different ratio from our artificial degrees. The refults are exhibited in the following table:

Heat of the water.	Ratio of Evaporation.
54°,5	1
77,	3
99,5	6,5
124,	12,
144,	20,5
167,	32,5

THE degrees of heat were taken by Lambert on Reaumur's fpirit thermometer, and the evaporation eftimated by lines of the Paris

 inch,

inch, of which I have given the ratio, and have converted Reaumur's into Fahrenheit's fcale. It appears that between 54°,5 and 99°,5 evaporation is nearly proportional to the differences of the temperature of the water, for as 22,5 (the difference between 54°,5 and 77°) is to 3, fo 45, (the difference between 54°,5 and 99°,5) is to 6.

From a feries of experiments which I profecuted in London during the year 1785, I am inclined to think that, other circumftances being equal, the ratios of evaporation to the temperature of water, are as expreffed in the following table:

Heat of Water.	Evaporation.
32°	1 Grain
45	2,5
55	4,
65	5,
75	6,5

These experiments were made on water contained in a cylindrical glafs veffel, under a furface of 25,23 fquare inches, and containing 23,13 cubic inches. It was partially expofed to the wind and air between the hours of twelve and two o'clock in the afternoon, but fheltered from rain. In winter the initial heat of the water was generally a few degrees higher than that of the air,

air, and in fummer a few degrees lower; but of the variable influences of the winds, or of the faturability of the air, I can give no account, except that of this laft I may fay, that the hygrometer never ftood lower than 70°; but as thefe influences fometimes concurred with, and fometimes oppofed, the action of heat, it is probable that, in producing the general effect collected from a number of experiments, they balanced each other.

2°. WITH refpect to *furface*, it is now generally agreed, in confequence of the numerous and accurate experiments of Wallerius and Richman, repeated and confirmed by Lambert, that other circumftances relative to the air and to the water being alike, evaporation, in equal *times*, is exactly in the ratio of the extent of the furface which water expofes to the atmofphere; and hence it is, that agitation of the water, by increafing and varying the furfaces, promotes evaporation. Yet Mufchenbrock, in his notes on the experiments of the Academy *del Cimento*, relates fome, in which he found, as he imagined, that the depth of water concurred with the furface in promoting evaporation; fo that if the furfaces were equal, the evaporation would be as the cube-roots of the depths; thus evaporation, from a veffel eight inches deep, and filled with water, would be twice greater, than the evaporation from another veffel of equal furface in the aperture, but only one inch in depth. In this experiment both veffels were of lead,

lead, and placed near each other, three feet above the ſurface of the earth, and expoſed to the full action of the air, wind and ſun. 1 Collect. Acad. p. 142. But he adds, that when both were placed in a room, within his houſe, the daily evaporation from each was equal.

In the firſt experiment, a diſparity of temperature ſeems to me to have taken place during its continuance, which occaſioned the inequality of evaporation. The external air conſtantly varies its temperature: now ſuppoſing the initial temperature of both veſſels and of the water contained in them to be equal; and ſuppoſing the external air ſome degrees hotter or colder than the water in theſe veſſels, the ſmaller of the two veſſels, and alſo the water contained in it, muſt have acquired the temperature of the air ſooner than the water in the larger of the two, and thus its evaporation muſt ſooner have been checked, as will preſently be ſhewn in treating of the influence of the temperature of the air, and this check to evaporation muſt have been renewed after every change in the temperature of the air; whereas, the water in the larger veſſel muſt have, during each change, acquired the temperature of the air more ſlowly, a circumſtance which promotes evaporation, as will be ſhewn. Therefore, in the ſecond experiment within doors, where the variations of the temperature of the air were fewer, or none, the daily evaporation from each veſſel was nearly equal.

And

And thus we fee, that depth is a circumftance of no importance, except as far as it is connected with temperature. But if we fuppofe, during the progrefs of the evaporation, fuch a change in the temperature of the air as reduces it to an equality with that actually exifting in the larger veffel, and below that which already took place in the fmaller, (which may happen by the intervention of a cloud) in that cafe the evaporation of the larger will be checked, while that in the fmaller goes on. And thus we may explain fome feeming anomalies in the experiments of Wallerius.

This celebrated naturalift expofed to the open air, in the month of Auguft, two tin parallelopipeds filled with water, whofe furfaces were nearly equal, but the height of one of them was one inch, and of the other two inches, and found that the fmaller in three hours loft 87 grains, and the larger only 77; but in three hours more, the fmaller further loft 148,75 grains, and the larger 151. The total lofs of the larger in fix hours was 228 grains, and of the fmaller 235,75. But in the month of June he found the larger to lofe in feven hours 199 grains, and the fmaller only 183. That the inequality of temperature in thefe experiments was the true caufe of the inequality of evaporation, he rendered evident by another experiment, in which, having coated cylinders of unequal capacities with clay, to prevent the quick communication of heat, filled them with water, and expofed them to the air, he found the

evaporation

evaporation nearly proportional to the furfaces, in free contact with the air. 11 Coll. Acad. 142, 143, and 146.

THE influence of free contact with the air is farther illuftrated by the experiments of the Reverend Mr. Williams of Bradford, in New England, for having filled with water two cylinders of equal diameters and depth, and fupplying the quantity evaporated from one of them once a week, and that evaporated from the other once a month, he found the evaporation from that filled once a week one-third greater than that from the cylinder filled once a month, in the hotter feafon of the year, and fomewhat below one-third in the colder. 2 American Tranf. p. 118. The reafon of which difference was, that the evaporation during a month being greater than that during a week, the air during the laft three weeks of each month was obliged to fink deeper into the veffel than during the firft week; it was therefore more confined, and could not be fo quickly renewed, as that over the veffel that was kept full every week; and therefore during three weeks in each month its evaporation was checked. His veffels, though in fome degree expofed to the air and fun, were fheltered from the rain, and therefore in fome degree from the open air. It fhould alfo be remarked, that according to the beautiful experiments of Doctor Prieftly, common water expofed to the fun, for fome time yields oxygen air, which accelerates evaporation while this pro-
duction

duction lafts, and hence the evaporation from fuch water is often greater at firft than afterwards.

The moft ftriking proof, and the happieft application of the efficacy of multiplied furfaces in promoting evaporation, may be feen in what are called the houfes *of graduation*, where waters, containing only one per cent. of common falt, are fo far evaporated by the mere multiplication of furfaces, that the refidue contains twenty per cent. and in this ftate, and not before, they are expofed to heat. Such houfes are common in Germany and Switzerland. See Haller's account of the procefs in the Paris Memoirs for 1764.

The third circumftance relative to the ftate of water, and which affects its evaporation, is its *purity*. It is well known, that concentrated folutions of moft falts evaporate much more flowly than pure water, in the fame circumftances; but Wallerius has fhewn, that the evaporation from a folution of common falt, whofe fpecific gravity was 1,0466, and confequently containing fixteen per cent. was nearly equal to that of fimple water, after the falt was fully diffolved; but during its folution evaporation is diminifhed by reafon of the cold produced.

Hence we fee, that under equal furfaces, evaporation from the fea is as confiderable as that from lakes.

WALLERIUS alſo found, that ice evaporates in proportion to its ſurface and temperature, though leſs than water equally cold; and that it evaporates more during congelation than when its congelation is completed. And hence it is, that ice and water evaporate more than mere water of the ſame temperature, and ſo much the more as the air is colder, probably becauſe congelation is promoted by air in proportion as the air is colder; nay, evaporation is checked during a thaw. 11 Collect Acad. 263, &c.

THE evaporation of ice was denied by Baron, but it was aſcertained beyond all poſſibility of doubt by the accurate experiments of Sauſſure. Hygrom § 130. Baron however has proved, that ice expoſed to wind loſes conſiderably by abraſion, the wind carrying off its particles in an exceeding fine powder. Sauſſure alſo obſerved, that ice evaporates in air four or five degrees colder than itſelf.

THE *ſtate of the air* in contact with water comes next under our conſideration, as it materially affects the quantity of evaporation. Air may be conſidered either with reſpect to its denſity, or with reſpect to its *temperature*, or with that of its *affinity*, which again involves the conſideration of denſity, or with regard to its *agitation*, or with regard to its *electricity*.

AND,

And, 1°. with refpect to *temperature*, the air in contact with water may be either of the *fame* temperature as the water, or it may be *warmer*, or *colder*.

If both the water and the air be of the fame temperature, it appears by the experiments of Richman, that evaporation proceeds moft flowly, as in that cafe the air in contact with the water foon becomes faturate; and though moift air is fpecifically lighter than dry air, yet that happens only when both are of the fame temperature, or the dry air colder. Now air in taking up moifture is cooled by it, and therefore continues its contact with water, and thus the progrefs of evaporation is checked.

Again, if the air be *warmer* than the water, and its temperature between 60° and 70°, and if the water be 14° *colder*, or more, in that cafe there is no evaporation at all, but, on the contrary, the air depofits the vapours it had previoufly poffeffed; for Richman found the fcale in which the water was at firft fully counterbalanced, gradually to fink, and drops of water appeared on the fides of the veffel. But if the water be only 11° colder, it will evaporate indeed, but flowly. But if the air be at any temperature between 75° and 87°, then evaporation is prevented only when and while the water is 20 degrees colder, or more; and water in Richman's experiments was depofited on the fides of

the veſſel, undoubtedly from the air in contact with them. But an inferiority in the temperature of water, amounting to leſs than 20°, will not prevent evaporation, but only proportionably diminiſh it; it is always briſkeſt when the inferiority amounts only to $1\frac{1}{2}$, or 2 degrees.

THEREFORE, the nearer water approaches to the temperature of the air, and yet remains one or two degrees below it, the more evaporation is accelerated. This may help to explain ſome experiments of Achard, 15 Roz. Jour. p. 468.

RICHMAN alſo obſerved that when air is ſome degrees warmer than water, evaporation is for a few minutes ſlower than it becomes afterwards; but after a few hours it is again retarded. The reaſon of which ſeems to be, that the inferiority of temperature in the water is originally greateſt, but gradually diminiſhes, and thus evaporation is accelerated; but, at laſt, the water attains an equality of temperature, and then evaporation is retarded.

THIS, however, can rarely happen, but when the maſs of water is ſmall, or its depth inconſiderable, or the air in a ſtagnating ſtate.

2°. WHEN *air is colder* than the water expoſed to it, the reſult, with reſpect to evaporation, is exactly the reverſe of what happens when

when the water is the colder of the two. For Richman's experiments ſhew, that other circumſtances being ſimilar, evaporation in equal times (at leaſt in portions of time not exceeding ſeven hours) is ſo much the greater, as the difference between the temperature of the air and that of the water is greater. Or, in other words, the *evaporation is as the differences between the colder air and the warmer water.* Thus, if a difference of 92° produces an evaporation as 13,5, a difference of 68° will produce an evaporation of 9,46. This law, however, reſts on the ſuppoſition, that the temperature remains unaltered (otherwiſe a new calculation would be requiſite), which may be ſuppoſed nearly true, when large maſſes of water in contact with the air are cooled by it; they deſcend and give place to the lower and warmer water, while the air, warmed by contact with the water, riſes, and gives place to the colder which deſcends: but ſuch retrogade movements, whether of the water or of the air, are imperceptible, unleſs the differences of temperature be conſiderable in the different ſtrata of each; and with reſpect to air in particular, unleſs the ſpecific gravity of the lower ſtrata be inferior to that of the upper in a notable degree. The previous ſaturability of air, (that is, its diſtance from the point of ſaturation) muſt occaſion conſiderable differences in the evaporation of the water expoſed to it.

Lastly, if we ſuppoſe the temperature of the water conſtant, and the temperature of the air in one caſe ſome degrees hotter,

and

and in another cafe colder than water, by an equal number of degrees, it fhould feem, that in equal times the evaporation would be greater in the hotter air than in the colder, becaufe colder air is fooner faturated, requiring only to produce faturation a fmaller proportion of moifture; yet as, on the other hand, hot air depofits moifture on water fome degrees colder, this point is not yet abfolutely fettled.

SECTION II.

Of the Influence of Affinity.

THE next property of air materially concerned in promoting evaporation is its *affinity* to water. That this affinity is very ftrong, appears by the difficulty of depriving water of the air it contains, either by ebullition, or by the action of the air-pump, as Mr. De Luc has long fince fhewn. This affinity, neverthelefs, does not feem to me a chemical affinity, for water and air do not form a *tertium quid*, as bodies chemically united do; and they are in great meafure feparable by mere mechanical means, as by the action of the air-pump, which bodies chemically united never are, and hence, Sauffure very properly calls it, an hygrometrical affinity: yet it refembles chemical affinity thus far, that it acts by attraction, and promotes an union, at leaft of adhefion, and this attraction has its limit, which is alfo called *faturation*.

OF

Of ſaturation, or of any approach towards it, no ſatisfactory knowledge could be obtained previous to the diſcovery of a comparable hygrometer by Mr. Sauſſure; an invention which perhaps is equal in importance, and conſidering it conjointly with the inveſtigation of the numerous incidents to which it gave birth, was attended with greater difficulties, and required more ſagacity, than any diſcovery, (thoſe of Scheele excepted) made during the courſe of the laſt century.

This inſtrument conſiſts of a ſingle human hair, duly cleanſed, and kept in a ſtate of tenſion by the weight of a few grains, and rolled on a pulley to which an index is fixed. The hair is lengthened by moiſture, and the index drawn up; it is contracted by dryneſs, and the index drawn down; the weight prevailing in the firſt caſe, and the elaſticity of the hair in the ſecond caſe: heat alſo lengthens the hair, but as it alſo promotes the evaporation of the moiſture adhering to the hair, it by that means, upon the whole produces a ſtill greater contraction, and thus makes it indicate ſuperior dryneſs.

Fixed points both of the *maximum* of deſiccation, and of the *maximum* of moiſture, that is ſaturation therewith, were accurately ſettled, and even ſome degrees of ſuperſaturation noted. The *maximum* of deſiccation is not undoubtedly the *true maximum*; yet

yet however, ſince it far exceeds any variable degree of moiſture, ever diſcovered in air, it is fully ſufficient for all meteorological purpoſes.

THE *maximum* of deſiccation is marked o on the ſcale of this inſtrument*, and the *maximum* of moiſture, or point of ſaturation, 98°. To prevent confounding theſe with thermometrical degrees, which often accompany them, I denote them by Roman numerals. XXV. marks the greateſt dryneſs, which according to Sauſſure can never be found in the open air†. XL. the greateſt dryneſs ever obſerved by him; conſequently even L denotes a conſiderable degree of deſiccation. But LXX denotes a degree of moiſture often perceptible on glaſs.

HERE it muſt be carefully remarked, that hygrometrical degrees denote very nearly the *ratios* which the quantity of moiſture poſſeſſed by a *ſtratum* of air, bears to the quantity that would ſaturate that air in its *actual* temperature; and as this ſaturating quantity varies more or leſs in every degree of temperature, it follows,

* I have often produced a deſiccation far below o by means of ſulphuric acid highly concentrated.

† I dont know that the dryneſs of the ſcirocco or harmattan was ever explored by this inſtrument.

follows: 1°, that the *fame ratio*, or hygrometrical degree, will indicate a different faturating quantity, and a different abfolute quantity of moifture, in every different degree of temperature.

2d°. That if in a given *ftratum* of air the temperature be altered, while the quantity of moifture remains conftant, the ratio or degree will alfo be altered; becaufe the quantity neceffary to faturation is altered: therefore, if in this cafe the temperature be raifed, the index will be lowered, and mark a greater drynefs, becaufe the air is farther from faturation, and therefore relatively dryer; and, *vice verfa*, if the temperature be lowered, the index will afcend, and mark a greater degree of moifture, though its real quantity be the fame, becaufe the air is then nearer to the point of faturation than it was before.

This ratio of the actual degree to the faturating quantity is what I call the relative *faturability* of air.

Hence it follows that, at high temperatures, the fame hygrometrical degree denotes an abfolute quantity of moifture far greater than it does at low temperatures; becaufe, though the ratios to the faturating quantity are nearly the fame, yet the faturating quantity in high temperatures is far greater than in low temperatures, and a high hygrometrical degree in air at a *low* tempe-

rature may indicate a far ſmaller quantity of moiſture than a low hygrometrical degree in a *high* temperature.

SAUSSURE did not ſtop at theſe general indications. He has given us, 1° The *abſolute* quantities of moiſture, correſponding with each hygrometrical degree, contained in the French cubic foot (that is 1,210 Engliſh) at the temperature of 15°,16 of Reaumur, equal to 65,8 Fahr. or rather 65,4, for he neglects the fraction 0,16: 2do. the abſolute quantities of moiſture in the cubic foot, correſponding with every five degrees of the hygrometer, commencing at the XLth, at every fifth degree of Reaumur, commencing at — 10 = 9°,5 of Fahr: but on the accuracy of this table he does not ſo much rely, as on that of the firſt. Fortunately, extreme accuracy for the general purpoſes of meteorology is not requiſite: 3tio. he has given a table of the diminution of the menſtrual power of air when rarefied to the degrees denoted by barometrical heights. With reſpect to the principles on which this table is founded I do not however perfectly agree with him.

OF the firſt table I ſhall here tranſcribe ſuch degrees as appear to me moſt curious and uſeful.

TABLE

TABLE I.

Of the Weight of Moisture in a Cubic Foot, indicated by the Hygrometrical Degrees at 65° of Fahr. Barometer 28,77. (English).

Hygrometer.	Grains.	Hygrometer.	Grains.	Hygrometer.	Grains.	Hygrometer	Grains.
I	0,0304	L	3,4852	LXVII	5,8505	LXXXIV	8,7170
II	0,0643	LI	3,5902	LXVIII	6,0329	LXXXV	8,8850
XXXV	2,1702	LII	3,6976	LXIX	6,1971	LXXXVI	9,0530
XXXVI	2,2475	LIII	3,8072	LXX	6,3651	LXXXVII	9,2210
XXXVII	2,3254	LIV	3,9192	LXXI	6,5331	LXXXVIII	9,3890
XXXVIII	2,4041	LV	4,0335	LXXII	6,7011	LXXXIX	9,5570
XXXIX	2,4834	LVI	4,1502	LXXIII	6,8691	XC	9,7250
XL	2,5634	LVII	4,2692	LXXIV	7,0370	XCI	9,8930
XLI	2,6451	LVIII	4,3905	LXXV	7,2050	XCII	10,0610
XLII	2,7291	LIX	4,5141	LXXVI	7,3720	XCIII	10,2290
XLIII	2,8155	LX	4,6534	LXXVII	7,5410	XCIV	10,3970
XLIV	2,9042	LXI	4,8021	LXXVIII	7,7090	XCV	10,5650
XLV	2,9952	LXII	4,9597	LXXIX	7,8770	XCVI	10,7330
XLVI	3,0885	LXIII	5,1271	LXXX	8,0450	XCVII	10,9010
XLVII	3,1842	LXIV	5,3031	LXXXI	8,2130	*XCVIII	11,0690 fat^{n}.
XLVIII	3,2822	LXV	5,4873	LXXXII	8,3810	XCIX	fuperfat^{n}.
XLIX	3,3826	LXVI	5,6775	LXXXIII	8,5490		

*He originally fixed faturation at C; but as fome uncertainty attended this point, he afterwards thought it fafer to fix it at XCVIII.

As air at each degree of heat may contain various degrees of moiſture, from the extreme degree of dryneſs up to the extreme or ſaturating degree of moiſture, *every* hygrometrical degree may be found in *each* degree of temperature, and conſequently tables ſimilar to the laſt might be formed for each degree of temperature. To form ſuch tables with due accuracy would require the labour of many years. Hence Sauſſure, in the ſecond table, has only marked the quantity of moiſture in a cubic foot, correſponding with a few of the hygrometrical degrees at each 5th degree of Reaumur, from —10° to + 30° Reaumur.

The ſaturating quantity of moiſture in a cubic foot I call its hygrometrical *complement*. As Sauſſure has given the complements of only every fifth degree of Reaumur, I have interpolated four arithmetical means for the complements of the intermediate degrees, but to diſtinguiſh them from thoſe of Sauſſure, I have added S to theſe laſt.

I have alſo given Fahrenheit's degrees under thoſe of Reaumur.

This table I have contrived to fill by taking the ratios, each hygrometrical quantity, at each degree of heat, bears to the complement, as ſhewn in page 310, but I have calculated only to 80° of Fahr.

TABLE

Reaumur	10°	9°	8°	7°	6°	5°	4°	3°	2°
Fahren.	9°,5.	11°,75	14	15°,12	18°,5	20°,75	23°	25°,25	27°,5
Hygrometer	S					S			
XL	0,8971	,9371	,9789	1,0206	1,0625	1,1067	1,1595	1,2066	1,2593
XLV	1,0676	1,1509	1,1653	1,2151	1,2890	1,3171	1,3760	1,4375	1,4987
L	1,2197	1,289	1,2954	1,3871	1,4442	1,5047	1,5712	1,5915	1,711
LV	1,4116	1,476	1,5427	1,6085	1,6742	1,7414	1,8218	1,9029	1,984
LX	1,6411	1,7119	1,7882	1,8647	1,9412	2,0246	2,1116	2,2060	2,300
LXV	1,9204	2,008	2,0975	2,1868	2,2767	2,3691	2,4769	2,5878	2,698
LXX	2,2277	2,338	2,4336	2,5463	2,6437	2,7482	2,8768	3,0054	3,134
LXXV	2,5215	2,637	2,7542	2,8720	2,9896	3,1107	3,2525	3,3973	3,542
LXXX	2,8155	2,944	3,0770	3,2071	3,3390	3,4734	3,6325	3,7943	3,957
LXXXV	3,1095	3,253	3,3980	3,5425	3,6886	3,8361	4,0124	4,1919	4,370
XC	3,4035	3,562	3,7197	3,8708	4,0379	4,1987	4,3924	4,5889	4,785
XCV	3,6946	3,861	4,0325	4,2025	4,3781	4,5578	4,622	4,9751	5,188
XCVIII	3,8739	4,0549	4,2359	4,4169	4,5979	4,7790	5,0023	5,2256	5,448

T A B L E II.

f the Quantities of Moisture in a cubic Foot of Air at different Degrees of the Hygr

	4°	3°	2°	1°	0°	1°	2°	3°	4°	5°	6°	7°	8°	9°
75	23°	25°,25	27°,5	29°,75	32°.	34°,25	36°,5	38°,75	41,°	43°,25	45°,5	47°,75	50°	52°,25
					S					S				
67	1,1595	1,2066	1,2593	1,3101	1,3653	1,4260	1,4910	1,5532	1,6394	1,6843	1'7595	1,8379	1,9122	1,9927
71	1,3760	1,4375	1,4987	1,5589	1,6248	1,6710	1,7778	1,8485	1,4514	2,0045	2,059	2,1878	2,2813	2,3749
47	1,5712	1,5915	1,7112	1,7800	1,8563	1,9378	2,0251	2,1112	2,2636	2,2900	2,3913	2,2782	2,6047	2,7115
14	1,8218	1,9029	1,9844	2,0635	2,1483	2,2470	2,3420	2,4462	2,5836	2,6503	2,7723	2,896	3,0203	3,1442
246	2,1116	2,2060	2,3004	2,3923	2,4976	2,6050	2,7210	2,8368	2,996	3,0690	3,2145	3,3580	3,5011	3,6437
691	2,4769	2,5878	2,6980	2,8061	2,9226	3,0550	3,1910	3,3280	3,5210	3,6055	3,6124	3,9382	4,1064	4,2747
482	2,8768	3,0054	3,1341	3,2592	3,3903	3,5490	3,7071	3,8659	4,0840	4,1824	4,3792	4,5745	4,7700	4,965
107	3,2525	3,3973	3,5425	3,6847	3,8375	4,0125	4,1908	4,3688	4,6131	4,7342	4,9495	5,1710	5,3920	5,612
734	3,6325	3,7943	3,9574	4,1153	4,2850	4,4803	4,6800	4,8789	5,1523	5,2862	5,5232	5,7753	6,0220	6,268
361	4,0124	4,1919	4,3702	4,5457	4,7324	4,9489	5,1699	5,3903	5,6904	5,8381	6,1271	6,3796	6,6523	6,925
987	4,3924	4,5889	4,7852	4,9763	5,1797	5,4176	5,6598	5,9000	6,2295	6,3900	6,5325	6,9898	7,2885	7,587
578	4,622	4,9751	5,1884	5,3977	5,6227	5,8751	6,1370	6,3980	6,7545	6,9420	7,2491	7,5727	7,8963	8,319
790	5,0023	5,2256	5,4489	5,6722	5,8956	6,1701	6,4446	6,7191	7,0936	7,2731	7,6129	7,9527	8,2925	8,632

To fac

II.

Degrees of the Hygrometer and Reaumur's Thermometer.

	7°	8°	9°	10°	11°	12°	13°	14°	15°	16°	17°	18°	19°
,5	47°,75	50°	52°,25	54°,5	56°,75	59°,	61°,25	63°,5	65°,75.	68°	70°,25	72°,5	74°,75
				S					S				
595	1,8379	1,9122	1,9927	2,0779	2,1710	2,2675	2,3649	2,4554	2,5634	2,7421	2,7973	2,9144	3,037
59	2,1878	2,2813	2,3749	2,4729	2,5830	2,6991	2,8140	2,9496	2,9952	3,2640	3,3298	3,4714	3,615
913	2,2782	2,6047	2,7115	2,8251	2,9575	3,0120	3,2130	3,3372	3,4852	3,7267	3,8018	3,9643	4,127
723	2,896[illegible]	3,0203	3,1442	3,2696	3,4206	3,5753	3,7260	3,8694	4,0335	4,3212	4,4082	4,5966	4,789
145	3,3580	3,5011	3,6437	3,7737	3,8523	4,1422	4,3192	4,4856	4,6554	5,0094	5,1103	5,3286	5,548
124	3,9382	4,1064	4,2747	4,4480	4,6519	4,8584	5,0659	5,2612	5,4873	5,8804	5,9948	6,2499	6,507
792	4,5745	4,7700	4,9652	5,1596	5,4023	5,6310	5,8910	6,1112	6,3651	6,8247	7,250	7,4563	7,622
495	5,1710	5,3920	5,6129	5,8404	6,1067	6,4679	6,6516	6,9080	7,2050	7,7146	7,8700	8,2063	8,544
232	5,7753	6,0220	6,2689	6,5213	6,8210	7,1249	7,4290	7,7155	8,0450	8,6163	8,7899	9,1652	9,542
271	6,3796	6,6523	6,9250	7,2022	7,5332	7,8709	8,2072	8,5230	8,8850	9,5179	9,7100	10,125	10,543
325	6,9898	7,2885	7,5872	7,8831	8,2550	8,6205	8,9840	9,3304	9,7250	10,417	10,630	11,084	11,540
491	7,5727	7,8963	8,3196	8,5640	8,9429	9,3421	9,7410	10,1153	10,5650	11,297	11,525	12,045	12,512
129	7,9527	8,2925	8,6323	8,9725	9,3918	9,8111	10,2304	10,6497	11,0690	11,5865	12,1040	12,6215	13,139

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r's Thermometer.

13°	14°	15°	16°	17°	18°	19°	20°	21°
1°,25	63°,5	65°,75.	68°	70°,25	72°,5	74°,75	77°,	79°,25
		S					S	
2,3649	2,4554	2,5634	2,7421	2,7973	2,9144	3,0370	3,1625	3,3033
2,8140	2,9496	2,9952	3,2640	3,3298	3,4714	3,6151	3,6952	3,9320
3,2130	3,3372	3,4852	3,7267	3,8018	3,9643	4,1276	4,2997	4,4892
3,7260	3,8694	4,0335	4,3212	4,4082	4,5966	4,7899	4,9761	5,2055
4,3192	4,4856	4,6554	5,0094	5,1103	5,3286	5,5481	5,7434	6,0345
5,0659	5,2612	5,4873	5,8804	5,9948	6,2499	6,5073	6,7697	7,0779
5,8910	6,1112	6,3651	6,8247	7,250	7,4563	7,6222	7,8526	8,7319
6,6516	6,9080	7,2050	7,7146	7,8700	8,2063	8,5442	8,8888	9,2935
7,4290	7,7155	8,0450	8,6163	8,7899	9,1652	9,5427	9,9251	10,380
8,2072	8,5230	8,8850	9,5179	9,7100	10,125	10,543	10,9614	11,466
8,9840	9,3304	9,7250	10,417	10,630	11,084	11,540	11,9977	12,552
9,7410	10,1153	10,5650	11,297	11,525	12,045	12,512	13,0340	13,609
10,2304	10,6497	11,0690	11,5865	12,1040	12,6215	13,1390	13,6558	14,2940

As the lower quantities of moifture, correfponding to the lower hygrometrical degrees at each degree of heat, bear to the *complement* of each degree of heat, refpectively, nearly the fame ratio, the blanks in the foregoing table were filled up with tolerable accuracy, (until the quantities are determined by experiment) by means of the firft table for temperature 65° Fahr. in which both the complement and the inferior quantities were accurately determined, and which I therefore call the *ftandard table*.

FOR, *as this ftandard complement for* 65°, *is to any inferior quantity of moifture, at any hygrometrical degree in that table; fo is the complement for any other degree of heat to the inferior quantity of moifture appertaining to the fame hygrometrical degree, at that given degree of heat*, at leaft fufficiently near.

Stand. Compl.	(at L.)	Compl. 32°	(at L.)

THUS as 11,069 : 3,4852 : : 5,8956 : 1,842. Sauffure found this laft 1,8563. But in higher degrees of heat the approximation is greater.

Stand. Compl.	(at XCV.)	Compl. 99°	(at XCV.)

THUS as 11,069 : 10,565 : : 20,784 : 19,8376, and Sauffure found it 19,8379.

To

To form this table from actual experiment from the XL. of the hygrometer to XCVIII. and from the 9th of Fahr. to 99°, would require 5220 experiments, of the niceſt and moſt delicate kind.

Saussure has given various other uſeful tables, for which I muſt refer to his treatiſe; but he has given one, to the principle of which I cannot entirely aſſent. He is of opinion, that the menſtrual power of air diminiſhes with its denſity, § 146; and hence infers, that its power of *receiving* vapour is alſo diminiſhed when it is rarefied. This laſt point, however, is contradicted by the quick evaporation that takes place in *vacuo* and on lofty mountains.

His own experiments ſeem to me to prove this increaſe of the receptive power of air when its denſity was diminiſhed; for he conſtantly found his hygrometer to move to dryneſs during every ſucceſſive exhauſtion, and permanently ſo, when the acceſs of new vapour was prevented, § 145. Hence it appears, that the greateſt part of the moiſture, originally contained in his hygrometer, being no longer confined by the denſity of the ambient air, gradually evaporated.

Mr. Pictet's experiments on the manner in which hygrometers are affected by mere vapour excite much ſurprize, and deſerve much attention. Eſſais de Phyſ. § 111.

He

He filled with vapour a receiver previoufly exhaufted of air; the receiver contained an hygrometer which ftood at XCVIII, and confequently marked faturation with moifture; and the temperature, both under the receiver and in the room, was 41°; no moifture was depofited on the glafs. The receiver, in this ftate, was carried into another room, whofe temperature was 32°; here moifture, and even drops of water, immediately appeared on the glafs, and the hygrometer indicated greater drynefs, for it ftood at XCI. And fo I think it fhould, for the quantity and denfity of what vapour remained in the air were really, diminifhed, drops having been formed from part of it; but after twenty minutes, the hygrometer again moved to moifture, and finally ftood at XCVII and one-half, and the cold under the receiver was increafed fix degrees. This too, it appears to me, fhould naturally be expected; for, by the increafe of cold, not only the vapours in contact with the hair are more condenfed, but its affinity to moifture is alfo increafed; it therefore abforbs more of them, and confequently moves to moifture. On moving the receiver, in this laft ftate, into a room whofe temperature was 45°,5, the drops condenfed on its interior began to evaporate, and while this evaporation lafted, the hygrometer indicated an increafe of moifture, but foon afterwards it moved to drynefs, and in three hours the temperature, being then nearly 48°, it ftood at XC, becaufe the heat being 16° higher, diminifhed the affinity of the hair to moifture, and

and caufed part of it to feparate from its interior; at leaft I fee no other caufe.

ALTHOUGH the Englifh cubic foot be fmaller than the French, and the French grain fmaller than the Englifh, yet I have left the quantities given by Sauffure unaltered, becaufe I am perfuaded that even the Englifh cubic foot of air, at leaft in temperatures exceeding 60°. contains at every hygrometrical degree much more moifture than is indicated by thefe tables. This our author acknowledges, § 47, and well explains why the whole of the moifture contained in a cubic foot of air can never be extracted; befides it feems to me, that under the veffels in which his experiments were made, the air not having fufficient power to expand itfelf as it fhould, on receiving moifture, could not take up the whole quantity which, were it at liberty, it would take up in high temperatures, that is above 75°, and therefore appeared faturated, when it, if at liberty, would not be fo.

As both faturability in a given heat, and alfo an increafe of heat, promote evaporation, it may be afked, which of them exerts the greateft influence? This curious queftion Sauffure has beautifully refolved from obfervations on evaporation on the *Col de Geant*, compared with thofe made at *Geneva*, and by an ingenious calculation of the refulting effect of each of them. Voy. aux. Alpes, § 2060.

THE

The ſaturability of air he eſtimates by the *number of grains*, over and above thoſe it already poſſeſſes, which it requires to arrive at ſaturation, in its actual temperature. Theſe grains he therefore calls *degrees* of ſaturability (or dryneſs). He thus found: 1°. that in air, whoſe denſity is about one-third ſmaller than uſual (as where the barometer ſtands at twenty inches) the influence or evaporating effect of a difference of one degree of Reaumur (2°,25 Fahr.) is ſomewhat more than three times greater than the influence of a difference of one degree of dryneſs or ſaturability.

2do. That in air of the uſual denſity, or only a few inches above or below it, the influence of a difference of one degree of ſaturability is about three times greater than the influence of a difference of one degree of Reaumur (2,25 Fahrent.) Therefore in an atmoſphere of the uſual denſity, on plains, the influence of a difference of nearly 7° of Fahrent. is nearly equal to the influence of a difference of one degree of ſaturability.

Hence our northerly and eaſterly winds, though ſome degrees colder than the ſoutherly and weſterly, yet promote evaporation much more; the air they convey being farther from ſaturation. Thus Achard found the evaporation from his atmedometer to be 13, on the 4th of May 1788, under an ENE. wind, thermometer 51°, hygrometer XLVIII. but on the 7th of the ſame month the evaporation was but 8, the wind being WSW. though the

mean heat of the day was nearly 60°, the hygrometer being at LVIII. and consequently nearer saturation. Mem. Berl. 1788, p. 126.

To give some idea of the saturability of air at each 5th degree of the hygrometer from XL. to XCV at any temperature of the air from $9^{\circ},5$ to $99^{\circ}5$, I have calculated the following table, which exhibits very nearly the ratio of the hygrometrical degrees to the complement or saturating quantity (at each degree of heat) taken as 1,000.

XL	0,2311.	Thus the complement of $9^{\circ},5$ is 3,8739, of which 0,2311 is between $\frac{1}{5}$ and $\frac{1}{4}$.
XLV	0,275	
L	0,314	
LV	0.364	
LX	0,422	
LXV	0,495	
LXX	0,575	
LXXV	0,650	
LXXX	0,726	
LXXXIII	0,80	
LXXXV	0,802	
XC	0,878	
XCV	0,952	

THUS

THUS Sauffure found the complement of 9,5 to be 3,8739 grains, and the quantity at XL. to be 0,8971 of a grain, and by the ratio here given it is 0,8952 of the complement.*

SECTION III.

Of the Influence of Wind.

THE third agent concerned in producing evaporation is agitation; whether of water in quiefcent air, or of air itfelf, that is *wind*, which neceffarily produces the agitation of the water expofed to it. It is evident that the agitation of water promotes evaporation by augmenting its furface to a degree that efcapes calculation. The agitation of air, befides involving that effect, promotes evaporation by the rapid removal of vapours as faft as they are formed, and by inceffantly prefenting new furfaces of lefs faturated air, and cooling the water if it be warmer than the air, in which cafe we have already noted the evaporation to be confiderable, or, if both be of the fame temperature, by lowering that of the water, or if the air be confiderably warmer than the water, by lowering the difference, and confequently promoting evaporation, as already fhewn. Moft of thefe effects of wind are proportioned to its faturability, of which enough has been already faid, and to its velocity, of which I fhall now treat.

* How obfervations fhould be made on this inftrument may be feen in the Appendix, Sauffure, 309.

Mr. Smeaton has communicated an excellent table of the velocity of wind, which I here tranfcribe from the Phil. Tranf. 1759, p. 165.

Miles per Hour.	Feet per Second.	Perpendicular Force on one fquare Foot, in Avoirdup. Pounds and Parts.	
1	1,47	,005	Hardly perceptible.
2	2,93	,020	Juft perceptible.
3	4,4	,044	
4	5,87	,079	Gently pleafant.
5	7,33	,123	
10	14,67	,492	Pleafant, brifk.
15	22,	1,107	
20	29,34	1,968	Very brifk.
25	36,67	3,075	
30	44,01	4,429	High wind.
35	51,34	6,027	
40	58,68	7,873	Very high wind.
45	66,01	9,963	
50	73,35	12,300	Storm or tempeft.
60	88,02	17,715	Great ftorm.
80	117,36	31,490	Hurricane.
100	146,7	49,200	Hurricane that tears up trees and carries buildings before it.

And

And Condamine tells us, that eighty-five feet per ſecond is the moſt violent ſtorm whoſe velocity can be meaſured at ſea. Mem. Paris, 1757, p. 637, 8vo.

Derham and Kraft's obſervations agree with thoſe of Smeaton. By means of this table*, if the velocity of wind be known by any inſtrument, its force may alſo be diſcovered, and *vice verſa*. The effect of the velocity of wind, inaſmuch as it reſpects evaporation, Sauſſure has detected, by new and ingeniouſly contrived, though indirect experiments, on *Col de Geant* and at *Geneva*, and found that air, moving at the rate of forty feet per ſecond, triples the quantity of evaporation that would happen in calm air. He has ſhewn, that in this caſe alſo, the greater or leſſer ſaturability of the air is a circumſtance that materially affects the magnitude of the effect. That on plains, a difference of one degree of ſaturability is equivalent to a difference of 11° Fahr. of heat; and on heights, on which the barometrical height is diminiſhed one-third, its influence is equal to that of heat.

Hence we ſee, that the warmer air is, the quicker its motion, the greater its ſaturability, and the longer its duration, the more it will promote evaporation.

Section

*The beſt portable inſtrument for meaſuring the force of wind is that invented by Dr. Lind, Phil. Tranſ. 1775.

SECTION IV.

Of the Influence of Electricity and Light.

THAT *electricity* powerfully aſſiſts evaporation is generally acknowledged. Sauſſure has ſhewn that a card, ſuperſatured with moiſture, loſt, when electrified, two grains in a quarter of an hour, while another equally circumſtanced, but not electrified, loſt only $1\frac{1}{2}$ grain: but he limits its action to water in a free and uncombined ſtate. Hygrometer: § 159 and 160. Briſſon alſo has obſerved, that an excited electric raiſes water above its level. Mem. Par. 1767, p. 328, 8vo. But how it acts in the operations of nature is but imperfectly known.

Light alſo contributes to evaporation by diſengaging air from water, and perhaps for other reaſons as yet imperfectly known.

SECTION V.

Of the joint Action of the above-mentioned Cauſes.

THE only ſeries of obſervations with which I am acquainted, made on evaporation, in which the principal cauſes that concur in producing it admit of a diſtinct conſideration, being marked by accurate inſtruments and preciſe meaſures, are thoſe of Mr. Achard

Achard, to be found in the Berlin Memoirs for the years 1788 and 1790. Of thefe I fhall tranfcribe a few, accompanying them with remarks, omitting the barometrical heights, as their differences, confifting only of two or three tenths of an inch, could not materially affect the quantity of evaporation.

1788	Mean Tem-peat ure.	Mean Hygrom. Degree.	Evaporation Ratio.	Wind.	Force Mean.
May 1ft	61,25	LII.	,001	NE.	5.3.
17th	56,	LIV.	,004	NE.	5.5
July 5th	72,5	L.	,014	SW.	5.5
1789					
July 2d	54,5	XLIX.	,001	W.	25.10

THE force of the wind is marked twice, namely at each period of obfervation, that is at feven o'clock in the morning, and at twelve o'clock. That which obtained during the remainder of the day is not noted, which renders thefe obfervations incapable of exact calculation, the degrees of the other inftruments and of evaporation.

evaporation were taken three times in twenty-four hours, namely at feven and twelve o'clock in the day, and at ten at night.

ON thefe obfervations I remark :

1o. THAT in the firft and fourth, the quantities evaporated were equal, though the temperatures of the air differed confiderably, viz. 6°,25, that, during the firft, being fo much warmer than during the laft. This equality can be attributed only to the greater faturability of the air in the laft cafe, and the greater force of the wind.

2do. THAT in the fecond obfervation, the evaporation was four times greater than in the firft, though the temperature was 5° lower and the hygrometer 3 degrees higher, yet the faturabilities at the refpective temperatures did not differ much, as may be feen by the foregoing table, and the force of the wind was much greater during the fecond obfervation.

3tio. THAT in the third obfervation, the evaporation was by far the greateft, though the force and duration of the wind were (at leaft to appearance) exactly the fame as in the fecond obfervation, becaufe the heat was 16° higher, and the hygrometer 4 degrees lower in the fecond obfervation.

THUS

Thus we see how observations duly made might be analysed, and the general result calculated *a priori*.

It deserves also to be remarked, that though Saussure never found the air in Switizerland drier than XL and frequently at XCV. or even saturated, yet Achard, at Berlin, during the years 1788 and 1799, never found it moister than LXX. generally between L. and LX. and frequently at XL. and once even at XXXII. Berlin stands on a sandy soil. The mean height of the barometer is 29,84 inches, and consequently its height over the level of the sea is about one hundred and forty feet, its distance from the Baltic one hundred and twenty miles, whereas Geneva stands on the border of a great lake. The mean height of the barometer is 28,77 inches, and consequently one thousand and ninety-one feet above the level of the sea, (others deem it one thousand two hundred and twenty) and about two hundred and twenty-five miles distant from the Mediterranean. The air in mountainous countries is known to be much moister than that over flat countries.

During the seven last months of the year 1785, and the seven first months of 1786, I made daily observations, and never found the hygrometer to stand lower than LXV. and that was on the 22d of April 1785, barometer 29,97, thermometer 56°, wind

NW. moderately ſtrong. The uſual ſtate of the hygrometer was from LXXV. to XC. and often ſtill higher.

THE greateſt loſs by evaporation which I ever obſerved in one hour, from my atmedometer, was forty-five grains, its ſurface was 25,23 ſquare inches, and it contained 23,13 cubic inches of water, barometer 30,00 inches, thermometer (which I always keep ſhaded from the ſun) 68°,5, hygrometer LXXIX. initial heat of the water 66°, final 64°, wind E. moderately ſtrong; and the ſmalleſt loſs in one hour was two grains, but frequently I could perceive no loſs during the firſt hour, and ſometimes ſome loſs during the firſt, ſcarce any during the ſecond, the water being frozen.

THUS on the 1ſt of March 1785, barometer 30,15, thermometer from 23° to 25°, wind N. initial heat of the water 32°, final 32°, but frozen hard, it loſt five grains the firſt hour and one on the ſecond hour, while frozen. The quantity of water in a cubic foot of air I found by ſulphuric acid to be 5,4 grains: I had then no hygrometer, but by Sauſſure's table it could not be far from ſaturation. But I have frequently obſerved a loſs of from ſix to eight grains during the firſt hour, thermometer 26°, and even ſix grains, when frozen, when the atmedometer was fully expoſed to the open air. But during foggy and calm days there

there was often no evaporation. Including every variable circumstance, I conclude the hourly evaporation at 32° may be estimated at four grains, and at 45° at ten grains, at 50° at twelve grains, at 55° at sixteen grains, at 65° at twenty grains, and at 75° at twenty-six grains, at a mean throughout the year, in countries where the mean hygrometrical height is LXXXV. and storms not very frequent. Hence we may calculate the annual evaporation from masses of water in countries whose mean annual temperature and hygrometrical state and agitation by tempests is known. This circumstance, however, has not hitherto been sufficiently attended to, nor consequently even approximately determined.

In London, whose mean annual temperature is about 50°, and where the hygrometer, at a mean of the year, is probably LXXXV. (I say probably, because its state in dry and moist years has not been determined) since the *hourly* evaporation may be stated at twelve grains, the *daily* may be stated at a mean of the year at two hundred and eighty-eight from a surface of twenty-six square inches, and therefore from one square foot one thousand five hundred and ninety-five grains, which multiplied into three hundred and sixty-five gives 582,175 grains = 83,1676 pounds avoirdupoise, nearly, or about 101,07 pounds troy. Such a quantity of water, in a vessel of one square foot in the bottom and

 aperture,

aperture, would rife to the height of upwards of 15,76 altitudinal inches, which is nearly the quantity of rain that falls in London in the drieft years.

Doctor Hales, from fome experiments, concludes the evaporation from a furface of earth to be nearly the fame in winter as in fummer, as it contains more moifture in the former feafon, and ftates its proportion to that from water as three to ten, but by *earth* he means earth divefted of vegetables, which fcarce ever occurs, 1 Hales, p. 55; and with his experiments thofe of the Reverend Mr. Williams, of Bradford, in New England, nearly agree, 2 Amer. Tranf. 120. But the evaporation from a fquare foot of earth covered with vegetables, and duly fupplied with moifture, is much greater than that from mere water, as follows from his experiments, and ftill more demonftrably from the great and fpeedy evaporation from the multiplied furfaces in houfes of graduation. In a cultivated country, like the vicinity of London, it is to that from water at leaft as fifteen to ten. Bazin, from two years obfervations on moift earth, infers that it evaporates much more quickly than an equal volume of water. Hift. Par. Acad. 1741.

Rain does not prevent evaporation, as I have frequently obferved.

Saussure's

Saussure's experiments on evaporation in tranquil air, both on Col de Geant, where the barometer ſtood at 20 inches, and at Geneva, where it ſtood at 28,77, bear ſome analogy to theſe, as they were not made on mere water, but on a piece of linen about thirteen inches long and ten broad, moiſtened until it weighed one hundred and fifty grains more than when dry, then expoſed to evaporation for twenty minutes and weighed.

On the Mountain.

Thermr.	Hygromr.	Saturability.	Evaporation.
51°	LXXIV	4,032	39,5 grs.
42°,5	XC	1,324	20,8
43°,5	LXXXV	2,184	24
			Mean 29,7
On the Plain.			
54,5	LXXXIII	2,495	19,75
48,76	LXXXIII	2,384	14,5
46,25	LXXXI	2,772	13,75
			Mean 16,00

Here

HERE two furfaces were expofed to evaporation, each confifting of one hundred and thirty fquare inches. The time of expofition was one-third of that of my experiments, but the furfaces were ten times greater. The mean temperature of the three experiments on the plain was 49°,5, the mean hygrometrical height LXXXII. and the mean quantity evaporated fixteen grains. This refult differs confiderably from the mean of mine made at nearly the fame temperature, but different degrees of moifture in the air, and under a furface ten times fmaller, and in a portion of time three times greater. If his furfaces and times were reduced to mine, then the evaporation would be but 4,8 grains; for fince two hundred and fixty fquare inches give fixteen grains, twenty-fix fhould give 1,6 grains; and this quantity multiplied into $3 = 4{,}8$ grains; the difference may proceed from the low faturability of the air in his experiments, whofe effect, as already faid, is three times greater than that of temperature, and poffibly from the attraction of the linen. I have found no obfervations among mine exactly comparable in their circumftances with his.

THE cold produced by evaporation is an effect deferving much attention in a meteorological point of view. In the months of July, Auguft and September, 1783, I made numerous experiments with a view of detecting the connection of this effect with the

the temperature of air in a tranquil ſtate, and the points from which the wind blew, but having no hygrometer I did not examine its ſaturability. The veſſel was the ſame which I employed in meaſuring evaporation already deſcribed; its ſurface twenty-ſix ſquare inches, the depth of the water one inch. The time of expoſure commonly five minutes. The water was ſheltered from the ſun, and almoſt always one or two degrees colder than the air. A thermometer was left in the water and compared with another ſuſpended in the air and ſhade cloſe by it. The experiments were made at two o'clock in the afternoon; I ſhall here ſtate a few:

Bar.

Bar.	Ther.	Wind.	Cold produced in the Water.	Time.
29,9	81°,5	N.	8°	3 minutes.
29.7	81°	NE.	12	5
29,55	79	S.	9	5
29,75	77	E.	6.5	4,5
29,7	77	W.	9,5	5
29,7	74	SW.	7	5
30,01	74	E.	8,5	5
29,9	74	NW.	9,5	5
30,0	72	N.	7	4
29,8	72	ENE.	10,5	5
30,05	72	W.	8,75	5
29,6	66	NNW.	5	5
29,75	65,5	NE.	5,5	5
29,7	61,5	S.	3,5	5
29,85	61,5	N.	3,5	4.5
30,1	56	E.	5	4,5
30,05	53	W.	3,5	5,
30,1	53,5	N.	5	5

HENCE

HENCE we fee, 1°. That when the temperature of the air is from 75° to 80°, and during a calm, the cold produced in water in five minutes is from 8° to 12°, even though the rays of the fun be intercepted:

2dly, THAT during the gentleft breeze from the fouth the cold is diminifhed, but if from the north or eaft, it is increafed:

3dly, THAT where the temperature is from 65° to 75°, the cold produced is from 5° to 8° :

4thly, IN temperatures from 53° to 65°, the cold produced is from 3° to 5° :

5thly, IT is plain that the cold produced is proportioned to the quantity of evaporation in a given time; when this quantity is very fmall, as twelve grains per hour, or one grain in five minutes, as in temperature 50°, in fuch circumftances the cold produced is fcarcely perceptible ; but if the water be expofed to a current of air, and this air unfaturated, the cold may be more confi-derable.

SAUSSURE, as ufual, made feveral ingenious experiments on the cold produced by evaporation in agitated air, both on a mountain

where the barometer ftood at 20 inches, and on a plain where it ftood at 28,77. It would be tedious to defcribe the particulars, but the general refult was, that when the velocity of the wind was about twenty-eight miles *per* hour, or forty feet *per* fecond, the cold produced by evaporation from a moift fpunge of about three fquare inches, in which a thermometer was inferted, was as follows*:

On the Mountain.

Thermr.	Hygromr.	Saturability.	Cold produced.
50°	LVII	6,799 grs.	19°
44,6	LXXXIV	2,328	3°
		On the Plain.	
69°	LI	7,478	20°,5
40°,1	XCI	1,082	2,25

Though various experiments have been inftituted to afcertain he cold produced by evaporation in evaporating liquids, yet I now of none wherein the cold produced in air, by the evapoation of liquids in contact with it, has been examined; hence with

* Voy. aux Alpes, § 2060.

with a view of determining this point I have been induced to make the following experiment, May the 25th, 1801, during rain and the wind E.

In a room 13,25 feet high, 24 feet long and 19 broad, whoſe cubic contents of air were therefore ſix thouſand and thirty-three feet, I ſuſpended by hooks from cords 7,5 feet high, and about the middle of the room, two ſheets previouſly wetted, but out of which the moiſture had been ſo far wrung that no drops fell from them, at the diſtance of 4,5 feet from each other, and placed a thermometer between them at the diſtance of about two feet three inches from each, and at the height of five feet, and another thermometer eighteen inches higher than the ſheets, and alſo a thermometer on each external ſide of the ſheets, at the diſtance of two feet on one ſide and eighteen inches on the other.

To eſtimate the loſs of moiſture in the ſheets by evaporation, I cut a ſquare foot of the ſame linen, which weighed, when dry, three hundred and fifty grains, and when wetted, and ſo much moiſture wrung out of it as not to drop, ſeven hundred and twenty-ſeven grains; it was ſuſpended by a wire from the ſcale of a balance.

The ſtate of the inſtruments, before the experiment began, was as follows: hygrometer LXXII. intermediate thermometer 62°, upper thermometer 61,5, lateral thermometers, each 62°.

The experiment began eight minutes before two o'clock, and at eight minutes after two I found the ſtate of the inſtruments as follows: the intermediate thermometer ſtill at 62°, the upper at 61°, and the lateral ones both at 61°. The hygrometer was unfortunately broken during the experiment.

The ſquare piece of linen, in the mean time, (that is during thoſe ſixteen minutes) loſt 23,25 grains of its weight; each ſide of it therefore loſt 11,625 grains. The dimenſions of each ſide of the ſheets were 8,5 feet in length, and 6,5 in breadth =35,25 ſquare feet; hence each ſide of each ſheet ſhould loſe 11,625 grains × 35,25=409,78 grains. But it is plain from the unvaried ſtate of the intermediate thermometer, that the evaporation from the internal ſides was obſtructed, that from each being obſtructed by that from the oppoſite ſide; and as the upper thermometer was lowered only half a degree while the lateral thermometers were lowered each one degree, it follows that each of the internal ſides evaporated only one-fourth as much as one of the external ſides, and both together half the quantity evaporated by each of the external ſides.

The

The influence of the evaporation of each external ſide of the ſheets extended at leaſt to the diſtance of two feet, as the thermometers indicate; each ſquare foot therefore on one external ſide lowered one degree the temperature of the two oppoſite cubic feet of air, and conſequently the evaporation from 35,25 ſquare feet lowered one degree the temperature of 72,5 cubic feet of air; but theſe 35,25 feet exhaled 409,78 grains of vapour. Therefore the reception of 409,78 grains of vapour in the temperature of 62° cooled one degree 72,5 cubic feet of air, or each cubic foot of air was cooled one degree by the reception and ſolution of 5,65 grains of vapour, and poſſibly by a ſmaller quantity, for I cannot ſay that the cold did not extend farther than the diſtance of two feet.

Now of the hygrometer in the room the temperature being 62°, the complement was therefore ten grains, and as it ſtood at LXXII. the ratio to its complement was 0,64; then as $10 \times 0,64 = 6,4$ grains, this muſt have been the quantity of vapour which the air originally contained in each of the two cubic feet; to which, if we add 5,65 we ſhall have 12,05, which exceeds the complement by 2,05 grains. It is probable then that theſe 2,05 grains were communicated to a greater diſtance, and that 3,6 grains only remained in each cubic foot. The ſolution therefore of 3,6 grains in a cubic foot of air in ſixteen minutes, and in the

the above circumſtances, cools it one degree. If the ſaturability of the air were greater, it would undoubtedly cool it more, and in a ſhorter time.

LEIDENFROST's experiments on evaporation in very high degrees of heat, ſince verified and extended by Ziegler, Sauſſure, Spalanzani, and D'Antic, and commented on by Lambert, are exceeding curious; but as they have no relation to meteorology, (and though I have myſelf repeated them), I ſhall not introduce them in this eſſay, but paſs to the ſecond object of enquiry relative to the moiſture of the atmoſphere, namely the ſtate of vapour when contained in it.

CHAP. II.

Of the State of Vapour ſubſiſting in the Atmoſphere.

VAPOUR or Moiſture in the atmoſphere may ſubſiſt in denſe air, or in air highly rarefied: that it is found in the former is well known, and that it may ſubſiſt in the latter appears by the obſervations of Bouguer, for he ſaw clouds three or four hundred toiſes above Chiromboracho, and conſequently at the height of twenty-two thouſand five hundred and twenty-eight Engliſh feet, or 4,3 miles over the level of the ſea; a height at which in the temperature of

of 32° a barometer would ſtand at 12,7 inches. At ſuch heights, and at much inferior, ſince evaporation proceeds much more quickly, it is not to be ſuppoſed that all the vapour ſo rapidly produced is diſſolved in the ambient air, but part riſes uncombined as it does under an exhauſted or half exhauſted receiver, and in this caſe Mr. De Luc's ſyſtem is admiſſible. This emiſſion of pure vapour ſeems to begin at heights at which the denſity of the air is 25 (that is at heights at which the barometer would ſtand at twenty-five inches, and thus I ſhall in future expreſs the various denſities of air,) at leaſt it is very conſiderable where the denſity is twenty, as already ſeen, p. 309. This leads me to treat of the properties and ſtate of pure inviſible vapour namely, its ſpecific heat, elaſticity, and ſpecific gravity.

The immortal Doctor Black, the father of all diſcoveries of this kind, informed me that the vapour of water, boiling at 212°, that is at 180° above the freezing point, and poſſeſſing the ſame ſenſible heat as the water, contains nine hundred and forty times more latent heat than an equal weight of water does heated to 212°, or 5,222 times more latent heat than it does of ſenſible heat, counting from the freezing point, for $180 \times 5,222 = 940$ nearly. In this caſe the preſſure or denſity of the atmoſphere is thirty, the barometer ſtanding at the height of thirty inches; and with Doctor Black's account the experiments of Mr. Schmidt of Gieſſen

Gieffen very nearly agree, for according to him the latent heat of the vapour of water, barometer 29,84 inches, and the heat 212°, is 5,33 times greater than its fenfible heat above the freezing point, now $180 \times 5{,}33 = 959{,}4$*. The difference or excefs in his experiment proceeds from the preffure of the atmofphere being fomewhat lower, as Mr. Watt's experiments prove.

Mr. Watt difcovered that the latent heat of fteam diminifhed in proportion as its fenfible heat increafed, Phil. Tranf. 1784, p. 335. Now the fenfible heat of fteam exceeds 180° above the freezing point when the barometer ftands above thirty inches, and is lefs than 180° when the barometer ftands lower than thirty inches, as Mr. De Luc firft difcovered, and may be feen in Sir George Schuckburg's and Mr. De Luc's tables, Phil. Tranf. 1779. p. 375. From thefe I have deduced the following table:

*4 Gren's Phys. Journal, p. 315.

Heat

Heat of boiling water.	
Bar.	Heat.
30	212°,
29	210,28
28	208,52
27	206,73
26	204,91
25	203,06
24	201,18
23	199,27
22	197,33
21	195,36
20	193,36
19	191,06
18	188,46
17	185,56
16	184,36
15	180,86
14	176,70

THE accuracy of this table even in the lower part of the ſcale is ſufficiently apparent by the reſult of the experiments of Sauſſure on ebullition on Mount Blanc; for on that enormous mountain, the barometer ſtanding at 16 French inches or 17,05 Engliſh, he found water to boil at the heat of 68°,993 of Reaumur, a degree which on Geneva thermometers is equal to 185°,56 Engliſh.—— Hence we ſee that diſtillation may be more advantageouſly effected on mountains than on plains, and at low barometrical heights than at the greater, yet within certain limits; for at heights that ſurpaſs 8 or 10 thouſand feet, the fuel, by reaſon of the rarity of the air, is more ſlowly conſumed. Hence alſo from the knowledge of the degree of the heat of ebullition to two or more decimal places, the ſtate of the barometer above or below 212° may infered to one or more decimal places.—The reaſon of this rapid diminution of the heat of ebullition below 25 inches is evidently the diminution of reſiſtance, from the diminiſhed weight of the atmoſphere, which then is very ſenſible; but as the cold continually produced by evaporation is then alſo very conſiderable, the *time* neceſſary to procure ebullition is longer as Sauſſure remarked on Mount Blanc. vol. vii. in 8vo. § 2011, p. 328. He found the heat of ebullition barometer 16 to be 68°,993 degrees, or in Engliſh meaſures barometer 17,05. 185°,5 of Fahr. (counting one of Reaumur at Geneva = 2,225 of Fahr.)

Hence ſince, according to Mr. Watt, the ſenſible heats of the vapours of boiling water at different barometrical heights are as the barometrical heights reciprocally, and the ſpecific heats of the vapours of water boiling are as the ſenſible heats reciprocally, it being known, that the ſpecific heat of the vapour of water heated to 180 degrees above the freezing point is 940. The ſpecific or latent heat of the vapour of boiling water, whoſe ſenſible heat is known, (and it may be known by the barometrical height as ſhewn in the above table and the notes) may alſo be diſcovered.

Thus the ſenſible heat of the vapour of boiling water barometer 30 being 180° above the freezing point (212°—32°=180°) and the ſpecific or latent heat of vapour, whoſe ſenſible heat is 208°,56 (that is 176,56 above 32°) as it is when the barometer ſtands at 28 inches, is 958 for ∷ 176,56. 180 :: 940.958*.

As pure inviſible vapour does not in my opinion (of which I have already ſtated the grounds) exiſt in the atmoſphere when its denſity is higher than 25, as it is in moſt of the inhabited parts of the globe, but is always in this caſe united to air, an enquiry into its latent heat at different temperatures below ebullition were ſuperfluous. But as it does exiſt in air whoſe denſity is 25 or leſs, ſince it is found in air whoſe denſity is 12,5, it becomes neceſſary to examine its latent heat in ſuch caſes, in all temperatures

* Hence 169206, being the product of 180°×940, is the common dividend of all ſenſible heats *below* 180°. when the latent heat of the vapour is ſought at barometrical heights below 30 inches.

tures inferior to that of ebullition. Now, by analogy, I apprehend this latent heat in all inferior temperatures may thus be determined:

As the *ſenſible* heat of ebullition, when the barometer is at 25 or below 25 is to the *latent* heat of the vapour at ebullition, ſo is the *ſenſible* heat of water heated to any inferior degree above 32° to the latent heat of its vapour, multiplied by 5 222. Thus the ſenſible heat of water in ebullition barometer 25 being 171°,4 (= 203°,4—32°) its ſpecific heat is 987 ($= \frac{169206}{171,4}$) the latent heat of the vapour of water at 22° above congelation (that is 52° on Fahr. ſcale) is 657 for ÷ 171°,4. 987 :: 22°. 126 × 5.22 = 657. The latent heat of vapour in ſuch caſes cannot be determined by experiment on account of the admixture of atmoſpheric air, we muſt therefore reſort to analogy, which in this caſe is perfect.

THE latent heat of pure vapour at greater heights is more conſiderable: thus at heights, at which the barometer ſtands at 20 inches, the latent heat of vapour whoſe temperature is 22° above 32°, as in the laſt caſe, is 730; for the heat of ebullition is 194°8, per table, = 162°,8 above 32°; and the latent heat of the vapour at ebullition is 1039. Now ÷ 162,8. 1039 :: 22. 140 and 140 × 5,22 = 730.

 As

As air is cooled by the reception of moifture diffolved in it, we muft infer that its capacity for containing heat is increafed, and hence moift air is more difficultly heated or cooled than dry air of the fame temperature. (For the cold proceeds from the abforption and not from the expulfion of caloric.)

The elafticity or expanfive force of pure vapour has been examined at every fifth degree of Reaumur above 0 to 110°. by Mr. Betancourt, and may be feen in an excellent work of Prony's, his *Architecture Hydraulique*, he has by a moft ingenious calculation interpolated the expanfions anfwering to the intermediate degrees. But Mr. Schmidt feems to have determined this expanfive force ftill more exactly than Betancourt. Hence I here infert his table, adding Fahrenheit's for Reaumur's degrees, and diftinguifhing the expanfions interpolated by calculation from thofe actually obferved by *I*. The forces are meafured by the elevation of a mercurial column in inches and hundreds of a French inch.*

Reaumur.

*The Paris cubic inch = 1,21 Englifh. Now the Englifh cubic inch of mercury when its fpecific gravity is 13,6 weighs 3443,2 Englifh grains, therefore the Paris inch weighs 4186 Englifh grains, and $\frac{1}{10}$ of this Inch = 418,6 grs: and $\frac{1}{100}$ of this inch 41,86 grains.

Reaumur.	Fahrenheit.	Expansive Force.	Reaumur.	Fahrenheit.	Expansive Force.
1	34°,25	,01 *I*	21	79°,25	1,01 *I*
2	36,5	,03 *I*	22	81,5	1,01
3	38,75	,05 *I*	23	83,75	1,19 *I*
4	41,	,07 *I*	24	86,	1,29 *I*
5	43,25	,11	25	88,25	1,30
6	45,5	,15	26	90,5	1,38 *I*
7	47,75	,16 *I*	27	92,75	1,42
8	50,	,20 *I*	28	95,	1,60 *I*
9	52,25	,25 *I*	29	97,25	1,80 *I*
10	54,5	,28	30	99,5	1,93
11	56,75	,34 *I*	31	101,75	1,02 *I*
12	59,	,38	32	104,	1,12 *I*
13	61,25	,44	33	106,25	2,23
14	63,5	,50 *I*	34	108,5	2,40 *I*
15	65,75	,55	35	110,75	2,68
16	68,	,61	36	113,	2,80 *I*
17	70,25	,69 *I*	37	115,25	3,20
18	72,5	,76	38	117,5	
19	74,75	,84 *I*	39	119,75	3,40
20	77,	,90	40	122,	3,64
			80	212,	28,

Note.

Note.—1°. Moſt of the interpolations from the 88th degree to the 122nd I have myſelf inſerted as thoſe calculated by Schmidt erred too widely by his own account. 4 Gren Phyſ. Jour. 273.

2do. Mr. Pictet has alſo made a ſet of curious experiments on the elaſticity of pure vapour in low temperatures. *Eſſais de Phyſique* p. 157. He found that a grain of warm water in *vacuo* evaporates in forty minutes in the temperature of 38° Fahr. under a receiver containing 1452 Engliſh cubic inches,* but that it did not diffuſe itſelf equally in leſs than ſix hours, and then raiſed the hygrometer from 17° to 60° that is 43°, and during this whole time the cold under the receiver was conſtantly decreaſing, though ſlowly, which decreaſe undoubtedly contributed to the diffuſion of the vapour.

Mr. Schmidt has alſo made a ſeries of experiments upon the dilatibility of air, made as *dry* as poſſible by expoſure to hot tartarin, an object of great importance, that had never before been examined. This table I here inſert, converting Reaumur's degrees into thoſe of Fahr. and adding from his formula the degrees he omitted.

Expanſion

*Ibid. page 91.

Expansions of dry air.

Reaumur.	Fahr.	Expansion of one Inch at 32°.	Reaumur.	Fahr.	Expansions of one Inch at 32°.
1	34°,25	,0044675		79°,25	,0938175
	36,5	,0089350		81,5	,0982850
	38,75	,0134025		83,75	,1027525
4	41,	,0178700	24	86,	,1072200
	43,25	,0223375		88,25	,11116875
	45,5	,0268050		90,5	,1116155
	47,75	,0312726		92,75	,1206225
8	50,	,035740	28	95,	,1250909
	52,25	,0402075		97,25	,1295557
	54,5	,0446750		99,5	,1340250
	56,75	,0491425		101,75	,1384925
12	59,	,0536100	32	104,	,1429600
	61,25	,0580775		106,25	,1474275
	63,5	,0625450		108,5	,1518950
	65,75	,0670125		110,75	,1563625
16	68,	,0714800	36	113,	,1608300
	70,25	,0759475		115,25	,1652975
	72,5	,0804150		117,5	,1697650
	74,75	,0848825		119,75	,1742825
20	77,	,0893500	40	122,	,1787000
				212,	,3574000

Note.

Note.—1o. HENCE we fee that 1000 inches or meafures of dry air at 32° would become 1004,4675 at 34,25 Fahr. and at 50° would become 1017,87. Hence 1000 meafures of *dry* air gain 1,985555 &c. by each degree of Fahr. above 32° (or more compendioufly 1,9856 which is true to two decimal places) or nearly two.

2do. WE fee the fource of the difcordant refults of D'Amontoms, De Luc, Lambert, Schuckburg, Roy, Berthollet, and Monge, &c. for they all operated upon air more impregnated with various degrees of moifture; befides taking the boiling point at different baromtrical heights; in the prefent experiments it was taken at 29,841 Englifh inches.

3io. It appears that the expanfions are as the differences of heat above 32° as D'Amontons, Lambert and Schuckburg alfo noticed, though their experiments, not being made on perfectly dry air, could not be very exact.

The dilatation of the moifture contained in air has been feparately examined by Mr. Schmidt, and he has fhewn how from it the volume of air faturated with moifture, faturated I fay at every degree of Reaumur, may be difcovered; the refult of his experiments appear in the following table of the volume which 1000 meafures at 32° of

of air would acquire if *saturated with moisture* at each degree of Reaumur above 32° expressed on Fahrenheits scale.*

Reaumur.	Fahr.	Expansive Force.	Reaumur.	Fahr.	Expansive Force.
1°	34°,25	1010,56		70,25	1122,68
	36,5	1010,78		72,5	1132,25
	38,75	1016,45		74,75	1142,53
	41,	1022,21	20	77,	1152,83
5	43,25	1028,58		79,25	1164,02
	45,5	1034,97		81,5	1175,23
	47,75	1040,41		83,75	1186,52
8	50,	1048,52	24	86,	1198,59
	52,25	1056,26	25	88,25	1211,44
10	54,5	1064,72		90,5	1223,65
	56,75	1071,28		92,75	1279,62
12	59,	1078,52	28	95,	1377,09
	61,25	1087,11		97,25	1494,02
	63,5	1095,76	30	99,5	1610,02
15	65,75	1104,46		101,75	1725,49
16	68,	1113,21	32	104,	1849,96
			33	106,25	1983,42

*To prevent mistakes, it must be noted that this table is not meant to express the dilatations of air saturated at any particular degree of heat it would acquire at other superior degrees, but only the bulk that 1000 parts dry air at 32° would acquire by saturation at each higher degree.

Note.—1o. Hence we fee that air faturated with moifture at high heats is much more expanded than dry air of the fame temperature, as De Luc and General Roy have alfo obferved, but in temperatures below 36°,5 dry air is more dilatable which probably induced Sauffure to conclude it was fo at higher temperatures. At 54,5 the difference is very perceptible for 1000 parts *dry* air at 32° are expanded at 54°,5 that is by 22°,5 above the freezing point to 1044,67, whereas 1000 parts of air faturated with moifture, are extended to 1064,72 and in higher heats, the differences of expanfion are incomparably greater.

2do Hence it is plain why moift air, fuch as that of the Weft Indies is much more fuffocating than dry air of the fame temperature. For 1000 cubic inches of air faturated with moifture at 86° of Fahr. contain nearly 76 inches of moifture which is ufelefs to refpiration.

3io. Thefe experiments agree with thofe of General Roy in which fteam was introduced at hazard, for the General found that from 32° to 52° each degree gave at a mean 2,588 and confequently thefe 20° would expand 1000 inches to 1051,76, and by Schmidt's experiments much more accurately made, we have 1050,33.

4o. SCHMIDT

40. SCHMIDT alſo obſerved a peculiarity in the expanſion of moiſt air, previouſly noticed by Roy, for Schmidt found that the expanſibility of air, ſaturated with moiſture, was ſmaller than the expanſibility of pure vapour, until the 167th degree of Fahr. but in higher degrees they conſtantly approached nearer to each other. And the General obſerved that the mean rate of expanſion, which from 152° to 172° of Fahr. was 12 for each degree, did from the 172° to the 192° increaſe to 17,88 for each degree, and increaſed ſtill more after the 192d to the boiling point. The ſluggiſhneſs of expanſion of air, ſaturated with moiſture at about 32°, was alſo noticed by the General, and he hence concludes the mean rate of expanſion from 0 to 32° of Fahr. to be 2,27 for each degree, which is ſmaller than that of drier air.

THESE variations of the rates of expanſibility of moiſt air, ſaturated at different temperatures, Schmidt very juſtly attributes to the variation of the degrees of affinity or adherence of air and vapour to each other at different temperatures. At 32° Fahr. it is very ſtrong, and alſo below that degree; and hence the ſtrong ſolvent power of air, colder than the water it acts upon, remarked by Richman; but if both are equally cold very little moiſture will be taken up by the air, as already mentioned; and hence I have ſaid that air diſſolves vapour when this is in a naſcent ſtate.

But in heats above 167° or 170° air and vapour are diſpoſed to ſeparate.

5°. HENCE we may deduce the impoſſibility of diſcovering a coefficient univerſally applicable to expreſs the rate of expanſion of air in every ſtate of moiſture, (as Tremley has well noticed. See 2 Sauſſure voy. aux Alpes 4°). This muſt vary with the mean ſtate of hygrometers above and below the heights to be meaſured; and experiments of this kind have not yet been made. De Luc's coefficient anſwers tolerably well for very dry air, that is whoſe ſaturability is greateſt, Sir George Schuckburg's for air much moiſter, and General Roy's for air ſtill more moiſt, that is whoſe ſaturability is ſmalleſt. Hence each ſucceeds in certain caſes, and fails in others. The dilatation or contraction, which air ſaturated with moiſture at any one given degree of temperature receives without the addition of any more moiſture, at any higher or lower degree of temperature, has not as yet been diſcovered; for Schmidt, who alone has attempted it, is juſtly diffident of the correctneſs of the table he has given of it, and in fact it is not grounded on the indication of any known hygrometer, and improperly ſuppoſes the L. degree to indicate the mean betwixt the loweſt and ſaturation. Whereas the LXVth degree on Sauſſure's indicates that mean; and XCVIII. and not C. indicates ſaturation.

ACCORDING

ACCORDING to Mr. Watt (as ſtated by De Luc, 3 Meteorolog: p. 145.) the ſpecific gravity of pure vapour is to that of air as 4 to 9. I ſuppoſe he compares it with air at the uſual denſity of 30 or 29, and at ſome particular temperature which is not mentioned; for at high temperatures the difference muſt be much greater, as appears by the foregoing tables.

MR. SAUSSURE, *Hygrometer*: p. 284, has given us the ſpecific gravity, not indeed of pure vapour, but of vapour diſſolved in air, with more preciſion, for he tells us, 1o. That a cubic foot of perfectly dry air has its volume augmented by $\frac{1}{54}$ when ſaturated with ten grains of moiſture at about 65° Fahr. of heat, and barometer 28,77 inches (Engliſh).

2do. THAT a cubic foot of *pure* or perfectly dry air of that denſity and at that temperature weighs 751 grains (French), and after diſſolving 10 grains of moiſture, by which it is dilated $\frac{1}{54}$, this new volume weighs $751 + 10 = 761$ grains; but a cubic foot of *pure* air augmented by an acceſſion of $\frac{1}{4}$ of its bulk of pure air would weigh $751 + \frac{1}{54}\ 751 = 765$ grains, that is 14 grains more. Hence he infers that in this caſe the ſpecific gravity of the diſſolved moiſture is to that of dry air as 10 to 14, for $\frac{1}{54}$ of a cubic foot in the one caſe weighs 10 grains, and in the other 14 grains nearly.

BUT

BUT I ſtrongly ſuſpect that the original experiment, on which this calculation is founded, is erroneous, chiefly by reaſon of the ſtrong adherence of moiſture to cold glaſs, as will hereafter be ſeen in treating of dew. From Schmidt's experiments, it may be inferred, that the ſpecific gravity of vapour diſſolved in air at this temperature is much lower with reſpect to that of pure air than Sauſſure has ſtated, for he tells us that about 1066 meaſures of dry air in temperature 65° would, if ſaturated with moiſture at that temperature, occupy the ſpace of about 1100 meaſures, and conſequently receive an augmentation amounting to about $\frac{1}{32}$ of their bulk; now, transferring this ratio to the cubic foot in Sauſſure's experiment, it appears that $\frac{1}{32}$ of a cubic foot thus added to the cubic foot of dry air weighs 10 grains; but a cubic foot of dry air, augmented by an acceſſion of $\frac{1}{32}$ of ſimilar air, would weigh 751 + 23,46 grains, which approaches nearly to Mr. Watt's ratio, therefore the ſpecific gravity of vapour diſſolved in air at this temperature is to that of perfectly dry air as 10 to 23,5 nearly. It ſhould however be recollected that Mr. Sauſſure found that a cubic foot of dry air in reality took up 11,069 grains of moiſture when ſaturated at this temperature, and that it was only by way of conceſſion to thoſe againſt whom he argued, that he ſtated the weight taken up at 10 grains; then we ſhould have of 11,069 to 21,195, or in round numbers as 11 to 21 or 10 to 19. And it ſhould farther be remarked, that the temperature is given very looſely,

loofely, for it is ftated to be from 14 to 15 or 16 degrees of Reaumur. See *Hygrometer*: p. 104 and 284.

SAUSSURE has given us a table, by the help of which the abfolute quantity of vapour at any barometrical height, in a cubic foot of air being known, the proportion and abfolute quantity in a cubic foot, at another barometrical height 3,6 inches lower, may be known from the mercurial height 28,77 to that of three inches and one-half, nearly.

THIS table I here give, adapting it to our meafures.

Barometer.	Ratio.
28,77	1,0000
25,17	0,9528
21,57	0,8899
17,97	0,8264
14,37	0,7629
10,77	0,6887
7,17	0,6230
3,57	0,4311

THUS fuppofing the abfolute quantity of diffolved vapour at any temperature, and barometer 28,77 to be 10 grains per cubic foot, then the quantity of vapour at a height at which a barometer would ftand at 25,17 inches, would be $10 \times 0,9528 = 9,528$ and at the height at which a barometer would ftand at feven inches, the quantity in a cubic foot would be only $10 \times ,6230 = 6,23$. But ftill it is fuppofed that at thofe great heights, at which barometers would ftand fo low, that the air is of the fame temperature as the original experiment

riment is made at, namely in this cafe, as it is found at barometer 28,77 inches; but fince in reality air at great heights is generally much colder than below, to afcertain the real proportion of vapour at thofe heights it will be neceffary to find the quantity of vapour which a cubic foot of air is capable of holding at that temperature barometer 28,77, and the ratio which the quantity or weight of vapour actually found bears to the complement at that temperature. Then, 2do. to find the complement of a cubic foot of air at the temperature which prevails at the given barometrical height, and diminifh it in the fame ratio in which it was found diminifhed below, and finally diminifh it ftill farther in the ratio which that barometrical height demands. An example will fully explain this rule.

Thus Sauffure found, barometer 28,77 and thermometer 82° of Fahr. a cubic foot of air contained about 10 grains of moifture at Geneva. Now the complement of 82° is nearly 15 grains, and the ratio of 10 to 15 is $\frac{2}{3}$. Then at Mount Blanc, on the fame hour, the barometer ftood at 16° and the thermometer at 26°,8; the complement of a cubic foot of air at this temperature is 5,3 grains, which diminifhed in the ratio of 2 to 3 becomes 3,5, and this, farther diminifhed by the ratio which the barometrical height of 16 inches demands, namely ,78 = 3,5 × ,78 = 2,7 grains, by obfervation it was found to be 1,7; the difference is only

only one grain. Voy aux Alpes, § 2007. How the temperature which prevails at thofe great heights may be found, will be fhewn in the fequel.

THE celebrated Lambert, of Berlin, Mem. Berlin, 1772, has alfo given an eftimate of the proportion of vapour which prevails in the atmofphere at different barometrical heights, deduced from calculations founded on many fictions, fuch as that of an homogeneous atmofphere, of pure air diftinct from commcn air, and an erroneous fyftem of the afcent of heat; yet as it is much eafier in its application and in the inftance juft quoted, approaches very near the truth, I have calculated the refults of his fyftem, which is nothing more than that the quantity of vapour at different barometrical heights above the earth is in the ratios of the fquares of thofe heights. By an homogeneous atmofphere it is probable he meant fuch a ftate of the atmofphere as prevails in ferene unclouded weather, and it is certainly only in fuch an atmofphere that any calculation can be inftituted.

Table of the Ratios of the Quantities of Vapour at different barometrical Heights, the Quantity at the Surface of the Earth being given.

Barometer.	Ratio of Vapour.	Barometer.	Ratio of Vapour.	Barometer.	Ratio of Vapour.
30,	900	24,	576	12	144
29,5	870	23,	529	11	121
29,	841	22	484	10	100
28,5	812	21	441	9	81
28,	784	20	400	8	64
27,5	756	19	361	7	49
27,	729	18	324	6	36
26,5	702	17	289	5	25
26,	676	16	256	4	16
25,5	650	15	225	3	9
25,	625	14	196	2	4
24,5	600	13	169	1	1

Thus in the example laſt quoted, the quantity of vapour in a cubic foot at Geneva being 10 grains, barometer 28,77, the quantity

quantity on Mount Blanc, barometer 16, ſhould be ,309, for as 827,7 (= $\overline{28,77}^2$) is to 256 (= $\overline{16}^2$) ſo is 10 to 0,309, which differs from the truth by only 0,391 of a grain.

As vapours unite to air, partly through the agency of heat, and partly through that of affinity and of electricity, ſo they ſeparate from it, ſometimes from a diminution of that degree of heat which they poſſeſſed in their naſcent ſtate, ſometimes from a diminution of affinity, and ſometimes from an alteration in their electrical ſtate.

In their firſt degree of coaleſcence when ſeparated from air, they form aggregates of exceeding minute particles, ſeparated from air by the diminution of affinity, and alſo from each other by electrical atmoſpheres; theſe aggregates are of equal, and often lower, ſpecific gravity, than the air in which they are formed, and yet are viſible by reaſon of their opacity; when near the earth they are called *foggs*, *miſts* or *haze*, (which differ only in denſity) and when at greater heights, *clouds*.

Vapours iſſuing from water or moiſture warmer than the air to which they unite, are ſoon cooled by it and thence in great meaſure diſmiſſed; hence the morning miſts obſerved in ſummer and the winter miſts of the colder regions; evening miſts on the

contrary proceed from the ſuperſaturation of air with vapours previouſly diſſolved, ariſing from the ſupervening decreaſed temperature. The inferior ſtrata of the atmoſphere are ſcarce ever ſuperſaturated by vapours ariſing from water or moiſture warmer than the air into which they aſcend; for before the point of ſaturation can be attained, their affinity to the portion of air to which they are united is weakened, and thence exceeded by the unincumbered affinity of the ſuperior ſtrata, and this happens ſucceſſively on to the higher regions; but with diminiſhed activity, by reaſon of the diminiſhed denſity of the higher ſtrata, until their ulterior progreſs is checked by ſaturation; but as they are ſtill continually recruited from below, their quantity is at laſt ſo far increaſed that they coaleſce into clouds. Here the proceſs recommences, for from the ſurface of theſe clouds a freſh evaporation often takes place, which, after ſome progreſs, is again checked in its turn, and clouds are formed at a ſuperior height; theſe again give room to a further evaporation, and a new ſtage of clouds is formed, until the proceſs is at laſt arreſted by the intenſe cold of the ſuperior regions. But the mere cold of congelation is not ſufficient to arreſt it, for Bouguer informs us that clouds are formed 2500 feet above the lower line of congelation, and that ice itſelf evaporates, though cooled, ſeveral degrees below the freezing point, is well known. The diſtance of the particles, both of air and vapour, from each other, when ſo far rarefied as they

they muſt be in the ſuperior regions of the atmoſphere prevents their coaleſcence in any but the extreme degrees of cold.

Hence we ſee that in the warmer latitudes and ſeaſons, various ſtrata of clouds may be formed one above the other; Muſchenbroock atteſts, that even in Holland, in Auguſt 1748, he diſtinctly diſcerned three. Theſe diſtinct ſtrata, varioully electrified and otherwiſe circumſtanced, give occaſion to various phænomena, the detail of which would here be miſplaced.

The clouds which commonly crown the ſummits even of low mountains, and often announce rain, are cauſed by the near approach to ſaturation at thoſe elevations, and its actual attainment through the evaporation from thoſe ſummits. But the ſummits of the loftieſt mountains ever crowned with ſnow, are generally ſhrouded in clouds from the cold they impart to the air in contact with them, and the loſs of electricity conducted away from the vapours contained in that air, by the mountain.

The heights at which the loweſt clouds are formed are various in various latitudes and ſeaſons; greater in the warmer and ſmaller in the colder. In latitude 54° in Cumberland, Mr. Croſthwaite obſerved none lower than 2700 feet, and none higher than 3150 in

in the courfe of feveral years*. But this country being mountainous they are probably lower than in others under the fame parallel. Lambert, in Berlin, latitude 52°.32′, in the month of July 1773, found their height 7792 feet, thermometer 65°, and the barometer fomewhat below its mean height†. Schuckburg alfo remarks, that clouds frequently reft below the fummit of *Saleve*, whofe height is 2831 feet. Phil. Tranf. 1777, p. 538, and Gentil at Pondicherry, latitude 12°, obferved fome at the height of 10240 feet. 2 Voy. p. 79.

THE weight of clouds, Sauffure eftimates at one-third or one-fourth of that of the cubic foot of air in which they fubfift. Hygrometer. p. 270. When the barometer rifes, clouds are partly diffolved, as denfe air is a better folvent than rarer air, and partly rife higher in confequence of the increafed fpecific gravity of the inferior air; when the barometer falls the contrary takes place.

CHAP.

* D'Alton's Meteorological Obfervations, p. 41.
† Mem. Berlin, 1773, p. 44.

CHAP. III.

Of the Temperature of the Atmoſphere.

SECTION I.

Of the Temperature of the Summer Months.

THAT the heat of the atmoſphere is derived, not from the immediate action of the ſolar rays that traverſe it, but from the warmer and more ſolid bodies with which it is or has been in contact, is a fact which at preſent can admit of no diſpute. I have ſuſpended a thermometer and a feather about an inch over the focus of a moſt powerful burning lens, without producing the ſlighteſt increaſe of heat in the one or of motion in the other. Hence it is plain the air was no way affected by it. That the heat communicated to the atmoſphere is in general gradually diminiſhed in ſome proportion during its progreſs upwards, at leaſt in ſummer, is alſo a fact ſufficiently aſcertained by the teſtimony of thoſe who have aſcended to great heights, either on mountains or in baloons. But the ratio in which heat is diminiſhed in its progreſs upwards, has been variouſly ſtated, and the means by which this diminution is effected, appear to me to have been incorrectly determined.

EULER

Euler ſuppoſed heat to decreaſe during its aſcent in air in an harmonic progreſſion; but this opinion has been long abandoned. Lambert, in the Mem. of Berlin for , fixes the ratio of this decreaſe to be that of 17 to 12; but this aſſertion is refuted by Sauſſure, Voy. aux Alpes, § 929, and is indeed incompatible with the reſults of experiments made at different heights, meaſured barometrically. Sauſſure, Voy. aux Alpes, § 2051, thinks that in latitudes from 45° to 47°, heat decreaſes during its aſcent 2°,25, or more correctly 2°,23 Fahr.* degrees for every 640 Engliſh feet, during the ſummer months, from the level of the ſea to the ſummits of the higheſt mountains, and conſequently one degree of Fahr. for each 287 feet of height. Yet this determination, though limited within ſuch narrow bounds, is not always juſt, even within thoſe bounds, and frequently errs very widely, at leaſt when the diminution of heat is computed, not from the level of the ſea, which frequently cannot be known, but from that of adjacent plains, to the ſummits of mountains. According to this rule, the diminution is conſtant and invariable, let the heat below be ever ſo different, as the diminution is proportioned to the height only, which does not vary. But numerous obſervations atteſt the diminution of heat to vary almoſt every day and hour, thus Schuckburg found the diminution of heat on Mount Saleve on

* The boiling point of water being taken at Geneva when the barometer was at 28,77.

on one day 9°, on another 8°,9, on another 10°,5; whereas, calculating from the above rule, it ſhould always be 9°,9; ſo on the Mole he found the diminutions of heat on different days 7°, 5°,8, 6°,6, 7°,9; whereas, by the above rule, it ſhould be 14°,8 every day. Here the error amounts to the double, or more.

To attain a cloſer approximation, we muſt reſort to ſome fixed point above, whoſe temperature is known, and ſo diſtant, that its variations, if it undergoes any, ſhould, with reſpect to that diſtance, be inconſiderable; then, this diſtance being known, and having another fixed point below, namely, the degree of heat obſerved, and framing a ſet of equi-diſtant intermediate terms, we have the neceſſary elements of a decreaſing arithmetical progreſſion, which all obſervations concur in eſtabliſhing as that obſerved by heat in its progreſs upwards, during the ſummer months, and in ſerene weather.

The fixed point in the ſuperior region of the atmoſphere to which I here allude is the height of the upper term of congelation. This, as well as the lower term of congelation, was firſt announced by Bouguer, ſo long ago as Ann. 1749, though no uſe has ſince been made of either in meteorological enquiries.

THE *upper* term of congelation is that above which no *viſible* vapour aſcends, and its temperature is *conſtantly* at leaſt 32° of Fahr. Bouguer has ventured to lay down its height over the ſnowy mountains of Peru under the equator, but it is highly probable that over the ſea, and eſpecially over land much heated, its height is ſtill greater within the ſame latitudes, ſince its height is proportioned to the intenſity of the heat below. And hence, between ſun-ſet and ſun-riſe, if the heat below be much diminiſhed, it muſt deſcend in proportion to the diminution. Bouguer has determined its height only in a ſingle inſtance, but its importance has induced me to inveſtigate the variations of this height in all latitudes above latitude 5°. The principles which I employed in this inveſtigation may be ſeen in the *Eſtimate of the temperature of different latitudes*, publiſhed in 1787, p. 8. Since that time I have corrected the heights appropriated in that eſſay to latitude 55°, and every other 5° higher, in the northern hemiſphere, by taking the inverſe ratio of the height in latitude 45°, divided by the ſine of 30°, namely, 2. The intervals I have filled by taking arithmetical means. Without the tropics, it is evident that not only night and day, but alſo winter and ſummer, muſt vary this height, and, in the higher latitudes, very conſiderably; hence I have confined this determination to the ſummer months, namely, May, June, July, and Auguſt, without the tropics. During the winter months, October, November, January, and February, this line

line muſt frequently approach nearly to the lower line, yet they never coincide, or at leaſt very rarely, namely, when the cold of the lower air is ſo great that no viſible vapours are formed in it. In the month of April, and in part of that of October, this line is intermediate between its height in ſummer and that in winter. And during the whole year it muſt be taken as a mean between its heights over the oceans and over land.

THE *lower* term of congelation is ſituated at the height at which it freezes at night, though in day time, and particularly on ſunny days, it may ſurpaſs that point by ſeveral degrees within the tropics; though fewer without them, even in the ſummer ſeaſon. Thus Bouguer found the heat on the ſtony ſummit of Pinchinca, at whoſe height he fixes this term, to vary 17° of Reaumur, that is 38° of Fahr. from ſun-riſe until noon. Voy. p. xxxix.

HE alſo ſtates the height of this line in latitude 28° at 13240 feet, and Sauſſure, more correctly, at about 12170 feet; and I believe it ſtill lower.

IN the latitude of the ſouth of France, (that is 43° and 44°) Bouguer ſtates the height of this line to be about 10240 and 9600, but Sauſſure eſteems it lower, for he makes it between latitude 42° and 43°, (that of Canigou) to be only 9290 feet*.

* Voy. aux Alpes, p. 374.

ALL thefe determinations muft neceffarily vary according to the feafons, and according as the heights are computed perpendicularly to fea or land, nay even according to the different fituations of the land and its different temperatures.

THUS, in the German or Northern fide of Swifferland, its height is only 8312 feet, but on the Southern or Italian fide, not above ninety miles diftant, it reaches 8952 feet*. The height of *Col de Geant* furpaffes the height of this line, and yet the thermometer rofe 10° Fahr. about two o'clock†, but at night it fell beneath that line.

THESE variations render the *lower* line of congelation an unfit limit towards which the diminution of heat fhould be computed, and hence I fix the limit on the *upper* line of congelation, whofe variations are, refpectively, at leaft in fummer and ferene weather, much lefs confiderable.

Table

* Bertrand, Renouvellementdes terres, 251.
† Sauffure Voy. § 2051.

Table of the Height of the Upper Line of Congelation in the different Latitudes of the Northern Hemisphere.

N. Latitude	Feet.	N. Latitude.	Feet.	N Latitude.	Feet.
0	28000	21	24404	38	17393
5°	27784	22	24147	39	16801
6	27644	23	23890	40	16207
7	27504	24	23633	41	15712
8	27364	25	23423	42	15217
9	27224	26	22906	43	14722
10	27084	27	22389	44	14227
11	26880	28	21872	45	13730
12	26676	29	21355	46	13235
13	26472	30	20838	47	12740
14	26268	31	20492	48	12245
15	26061	32	20146	49	11750
16	25781	33	19800	50	11253
17	25501	34	19454	51	10124
18	25221	35	19169	52	8965
19	24941	36	18577	53	7806
20	24661	37	17985	54	6647

N. Latitude.

N. Latitude.	Feet.	N. Latitude.	Feet.	N. Latitude.	Feet.
55	5617	67	4616	79	3911
56	5533	68	4548	80	3861
57	5439	69	4480	81	3815
58	5345	70	4413	82	3769
59	5251	71	4354	83	3723
60	5148	72	4295	84	3677
61	5068	73	4236	85	3631
62	4989	74	4177	86	3592
63	4910	75	4119	87	3553
64	4831	76	4067	88	3514
65	4752	77	4015	89	3475
66	4684	78	3963	90	3432

THE height of this line in any latitude, divided by 100, gives the number of terms in the arithmetical progreffion, each confifting of 100 feet; and the difference of the heat below, and 32°, gives a dividend, which, divided by the number of terms, lefs one, gives the common difference of the progreffion*; and the common difference, multiplied by the number of terms in the height of a mountain,

* It is commonly a decimal fraction, of which the three firft fignificative figures are generally fufficient, but it is fafer to take four in fome cafes.

mountain, or other height fought, gives the diminution of heat at that height; and this diminution fubftracted from the heat below, gives the degree of heat that obtains at the propofed height.

EXAMPLE.

In latitude 56°. 30, the heat below being 54°, it is required to know the diminution, and the degree of heat that prevails at the height of 1364 feet?

Here the latitude being intermediate between latitude 56° and 57°, the height of the upper line of congelation is the mean between 5533 and 5439 = 5486 feet, then the divifor is 53,86 terms, and the dividend is $54^{\circ} - 32^{\circ} = 22$, and $\frac{22,00000}{53,86} = ,408$, the common difference. Now $0,408 \times 13,64 = 5,5$ the diminution, and $54 - 5,5 = 48^{\circ},5$, the degree of heat which prevails above; which is exactly that obferved by General Roy at the fummit of Knockfarle in Scotland. Phil. Tranf. 1777, the heat below being 54°, and the height 1364 feet.

In this manner I have calculated the heats at various heights in various latitudes, the degrees below being given, the refults both by obfervation and calculation may be feen in the annexed table.

Table

Table of the Heats observed and calculated on sundry Mountains.

Latitude	Heights.	Heat			Difference	Authors.
		Below.	Above by Observation.	Above by Calculation.		
0	Pinchinca 15564	83°,75	29°,75	30°,5	0°,75	Bouguer.
38°	Etna 10954	73,25	40,08	47,2	6,4	Saussure, Voy. 941.
46°	Saleve 2831	73,	64,	64,16	0,16	Schuck. Phil. Tran. 1777, p. 530.
46°	Mole 4211	63,9	56,	53,7	2,3	Schuckburgh.
46°	Col de Geant 11273	81,	41,5	42,5	1,0	Saussure, § 2035.
46°	Mt. Blanc 14624	82,8	26,82	26,44	0,38	Saussure, § 2003, p 307
53°	Snowden 3555	60,	47,75	47,1	0,65	Roy, Phil. Trans. 1777, p. 779.
53°	Moel Elio 2371	62,5	51,	51,3	0,3	Roy, ibid.
56°	Arthur's Seat near Edinburgh 803	54,	50,5	50,	0,5	Roy, ibid, p. 777.

56°

Latitude.	Heights.	Heat: Below.	Heat: Above by Obfervation.	Heat: Above by Calculation.	Difference	Authors.
56°	Kirk Yetton Cairn 1544	54°,5	47°,25	48°,3	1°,05	Roy, Phil. Trans. 1777, p. 777
56°,30′	Schihallien 3281	56,	5,	41,4	3,6	Roy, ibid.
56°,30′	Bolfrack 1076	60,	56,75	54,5	2,25	Roy, ibid.
56°,30′	Knockfarle 1364	54,	48,5	48,5	0,	Roy, ibid.
56°,30′	Glenmore 1279	55,	51,5	49,6	1,9	Roy, ibid.
56°	Tinto 1642	58,	51,	50,3	0,7	Roy, ibid.
46°	Dole 4810	78,25	65,	63,44	1,5	Roy, ibid, 783 & 736.
51°,31′	Kew Pagoda 116	49,25	49,25	49,05	0,2	Roy, ibid, 773.
80°	Hacklyyt Hill 1503	50,	42,	43,	1,	Phips' Voy.

HERE are eighteen calculations of heat at different heights, under different latitudes; among which only two differ remarkably from the heats actually obferved at thofe heights, and thefe I am inclined to attribute to an error in the obfervations, for the heats above and below cannot both be noted by the accurate philofophers who meafure the heights of mountains. The errors, if any are committed, in noting the degrees of Reaumur, are much more confiderable than thofe that can be committed in taking thofe of Fahrenheit.

THE near agreement of the calculated and obferved heat on the fummit of Mount Blanc is very remarkable, as the height of this mountain furpaffes that of the upper line of congelation, and is the only known height that does fo; that it furpaffes the upper line of congelation is evident from Sauffure's account; for he tells us that the air was fo free from vapour on this *fummit*, that in a fhaded fpot he could fee the ftars by day, and the temperature even in day time was far below 32°; the fnow that nevethelefs repofed on its fummit might for the moft part have been drifted on it by the furious winds that rage in winter. Thus Dr. Villars found thirty beds of earth interpofed between thofe of frozen fnow on the Glaciers of Swifferland. 22 Rozier, p. 275. This earth or duft was wafted up in the months of July and Auguft during thirty fucceffive years.

THE

THE clouds that frequently inveſt its ſummit originate from vapours condenſed by the abſorption of electricity by the mountain. It is alſo highly remarkable that the common difference of the progreſſion ſhould ſuit terms beyond the laſt, by the means of which it was found. It would therefore ſeem that the common difference thus found takes place at much ſuperior heights.

THE ſmaller differences between the degrees of heat calculated, and thoſe obſerved, may well be attributed to local circumſtances; *below*, in ſome ſituations of ſmall extent, *reflected* heat often occurs; *above* if the ſummit be graſſy the heat is diminiſhed in ſome degree by evaporation; if ſtony but narrow, the genuine heat is little affected; but if broad, as the ſtony ſummit of Pinchinca, a conſiderable degree of heat may ariſe from reflection, on ſunny days. Darker days ſeem more favourable to detecting the true heat appropriated to the height.

NOTWITHSTANDING the imperfections incident to obſervations on mountains, yet theſe are upon the whole much leſs exceptionable than thoſe that hitherto have been taken in balloons, of which the height is ſeldom determined with ſufficient preciſion; beſides ſimultaneous obſervations of the temperatures above and below rarely occur, and on ſunny days the heat reflected by balloons cauſes conſiderable errors.

LASTLY, it ſhould be noticed that the horizontal diſtance between the places of obſervation ſhould not exceed eight or ten miles.

BY means of this progreſſion ſeveral curious queſtions may be reſolved, the latitudes being given.

LET *H* denote the heat below, *h* the heat above, *D* the diminution, *d* the common difference or rate of the progreſſion, *n* the number of terms in the propoſed or ſought for height, which multiplied by 100 gives the height truly, to leſs than 20 feet.

L the height of the upper line of congelation, in the given Latitude.

THEN 1° $d = \frac{H-32}{L-1}$. 2do. $D = d\ n$. 3io. $n = \frac{D}{d}$. 4°. $H = h + D$.

$$5^\circ\ h = H - D. \quad 6^\circ\ L = \left(\frac{H-32}{d} + 1\right) \times 100.$$

THUS 1°. the heat below being given in latitude 56°,30′ to find the rate of its progreſſion upwards? Suppoſe the heat below 54°, then by the firſt formula $\frac{54-32}{54{,}86-1} = \frac{22}{53{,}86} = {,}408$; then it is diminiſhed ,408 of a degree in every hundred feet of its progreſs upwards.

THUS

THUS in the firſt hundred feet it becomes $54^\circ - ,408 = 53^\circ,592$. And at 200 feet it becomes $54^\circ - \overline{,408 \times 2}$, &c.

2do. THE *rate* of diminution being known, ſuppoſe ,408 to find its amount at a given height, ſuppoſe 1364 feet? By the ſecond formula we have $0,408 \times 13,64 = 5^\circ,5$.

3io. THE diminution of heat being known, ſuppoſe $5^\circ,5$, and alſo its rate, ſuppoſe 0,408, to find the height at which this diminution takes place? By the third formula we have $\frac{5.5}{0,408} = 13,48$ which $\times\ 100 = 1348$ feet, which wants but 15 feet of the truth.

HENCE we ſee that heights may often be meaſured by thermometors only with tolerable accuracy.

4o. THE heat *above*, ſuppoſe $48^\circ,5$, and its diminution, ſuppoſe $5^\circ,5$ being given, to find the heat *below*? By the fourth formula $48^\circ,5 + 5^\circ,5 = 54^\circ$.

5o. So alſo the heat *below*, and the diminution above, being known, the heat *above* may be found by the fifth formula as is evident. Thus 54° (in the laſt example)$-5,5 = 48,5$.

6o. THE

6°. THE heat *below* and the rate of progreſſion being known, the height of the line of congelation may be found by the ſixth formula. Thus in the example $\frac{54-32}{40846} + 1 = \frac{22,00000}{40846} + 1 =$ $53,86 + 1 = 54,86 \times 100 = 5486.$

Note.—That the heat *below* and the heat *above* being given, if the difference includes no fraction, and if the height alſo be known, the rate or common difference may be found by dividing the difference of heats above and below, by the height or $d = \frac{H-h.}{n}$ n being expreſſed in the terms of the progreſſion. Thus the height of Tinto being 1642 feet it ſhould be expreſſed 16,42.

SOME have thought that heat is conveyed upwards by the gradual aſcent of the lower ſtrata of air which firſt receive the increaſe of heat, but that this cannot happen will be ſhewn in the ſequel.

IN ſerene weather, whatever variations may occur in the temperatures below, the differences between them and the heats above are as nearly the ſame as can be expected, when due allowances are made, for the imperfection of thermometers on which fractions

tions of degrees are often had only by eſtimation, and alſo for local circumſtances.—A full proof that heat aſcends in an arithmetical progreſſion.

THUS in the three ſeries of obſervations on the temperatures at the top and bottom of *Mount Saleve*, the difference in the firſt was 8°, 9, in the ſecond 9°, in the third 10°, 5, here the greateſt deviation is 1°,5.

AND in the ſixth ſeries at the *Mole* the differences were, in the firſt 7°,1, in the ſecond 5°,8, in the third 7°, in the fourth 7°.9, in the fifth 7°; in the ſixth 6°,6. Here two circumſtances deſerve attention; firſt, that in the ſecond, third and fourth ſeries the heat above was conſtant, namely 56°, while the heat below varied from 61°,8 to 63°'9. This may be attrbuted to a briſk wind, from the glaciers which reigned above, while the lower flank of the hill was ſheltered. The ſecond is, that the difference between the heat abvoe and the heat below, with reſpect to Mount Saleve was 9°, and yet betwixt the top and bottom of the Mole it was at moſt only 7°9 though the height of Mount Saleve was only 2831 feet and that of the Mole was 4212. This muſt certainly proceed from the intervention of ſome contingent cauſe, the general differenee may therefore be often better determined by calculation than by actual obſervation.

IN

In a *given latitude*, the rate of diminution at equal heights, is greater when the temperature below is high, than when it is low; for while the divifor (viz. the height of the line of congelation) remains unaltered in all temperatures, the dividend (viz. the difference betwixt the heat below and 32°) is fo much the greater as the temperature is higher, and confequently the rate or quotient muft be higher. Sauffure alfo noticed this difference of the rate of diminution in high and low temperatures; for he tells us that in winter the rate of diminution is but $\frac{2}{3}$ of that which it amounts to in fummer. Voy. § 2051. And between the tropics heat rifes much more rapidly than in the higher latitudes, as Dr. Caffan obferved, 36 Roz. Jour. p. 265. See alfo Mr. Six's obfervations in the Phil. Tranf. 1788, p. 105. And hence the difference betwixt the temperature of mountains and of plains is not fo great in winter as in fummer. Nay in winter the temperature at great heights is often warmer than on plains, as will prefently be fhewn.

We owe to Mr. Pictet fome new and curious experiments on the progrefs of heat at low heights; that is between 5 and 75 feet. He found that a thermometer, fufpended about five feet over the earth, generally increafed in heat more rapidly than a thermometer placed 75 feet over the earth; fo that on the hotteft hour of the day, the lower was often about 4,5 degrees warmer than the upper thermometer. This fhews that the increafe of heat below is quicker than its communication upwards. However towards

towards ſun-ſet both thermometers approach to an equality. This ſhews that the higher thermometer at 75 feet communicates its heat upwards ſtill more ſlowly than the lower communicates its heat to it; which proves that the higher heat aſcends, the more ſlowly it is propagated to ſtill ſuperior heights. This explains the conſtancy of the heat at the ſummit of the mole while the heat below increaſed as already mentioned. But after ſun-ſet the lower thermometer cools quicker than the upper, and towards the end of twilight is often about four degrees colder, notwithſtanding that the earth a little below its ſurface is much warmer. The cold which thus affects the lower thermometer is evidently cauſed by evaporation; this exceſs of heat of the upper over the lower thermometer laſts the whole night, and until two hours after ſun-riſe, when, notwithſtanding the evaporation, the earth is more heated by the ſun's rays than it is cooled by evaporation, and ſo alſo is the thermometer ſuſpended five feet over it, and thus accumulates heat faſter than it can reach the upper thermometer, until after the hotteſt hour of the day, as already mentioned.

THESE obſervations happily point out the hours of the day moſt proper for diſcovering the mean temperature of the atmoſphere, a matter of great importance in barometrical menſuration. It is evident that the hour of ſun-riſe (or before ſun-riſe) is an improper time, ſince the heat at the height of 75 feet is ſome

degrees higher than that at five feet above the furface of the earth; contrary to the grounds of calculation. Nor is the hotteft hour of the day the fitteft time, fince the heat is above all proportion fome degrees greater than at the height of 75 feet. The propereft times are therefore the mean hours between fun-rife and the hotteft hour of the day*, and between this and fun-fet. *Effais d s Phys.* § 131, &c.

HOWEVER, fuch confiderable difparities occur only in calm, clear and ferene weather; on cloudy days, when the fun does not appear, they vanifh. Ibid. § 135.

WHENEVER a fog or mift appears in any ftratum of air, it communicates about a degree, or half a degree, of heat to the ftratum in which it is found. Ibid. § 142. A circumftance that deferves notice in eftimating the mean temperature of the atmofphere. Mr. James Six, of Canterbury, made a number of obfervations on the temperatures fix or nine feet above the earth; and alfo on that which prevailed 220 feet above it; and his refults were in general analogous to thofe previoufly obtained by Pictet. But the temperatures of whole days, or of days and nights only, were noticed, and not the fimultaneous differences at different heights; befides

* This I have added.

befides the heats below appeared to have been influenced by reflection, and the heat above by contact with the wall. Thus on the 16th of June, 1784, the heat below being 68°, the heat above was no more than 60°; fo great a diminution at fo fmall a height could not arife from the natural progreffion of heat, but muft have arifen from the range of a N E. wind which prevailed on part of that day from which the garden was more fheltered.

SECTION II.

Of the Temperature of the Winter Months.

BY winter months I underftand thofe of November, December, January and February. The temperature of thefe months has prefented fome extraordinary phænomena hitherto deemed inexplicable, or at leaft not fully accounted for, though they appear to me connected with and dependant on a fact fuggefted by that eminent philofopher and mathematician Dr. Halley, fo long ago as the latter end of the feventeenth century.

THE firft phænomenon is, that during the winter months, the temperature of the higher ftrata of the atmofphere is often warmer than that of the lower.

Thus the temperature of the ſummit of Arthur's ſeat, near Edinburgh, though only 684 feet above the baſe of Hawk-hill obſervatory, was, on the 31ſt of January, 1776, found to be 6° warmer than the temperature below. Phil. Tranſ. 1777, p. 777 and 728. At about eleven o'clock in the forenoon, M. De Luc tells us, that in winter he ſometimes found the temperature on the ſummits of mountains from 2500 to 3000 feet high, exactly the ſame as that of the plains. Modif. § 203. A circumſtance that never occurs in ſummer. Count Fraula, in the 3d volume of the Memoirs of Bruſſels, has ſhewn by experiments, that thaws begin above and are gradually propagated downwards; Meſſier, in the Memoirs of Paris. 1776, p. 19, having placed two thermometers, one at 20 feet above the ground and another 54 feet higher, obſerved this latter to ſtand conſtantly ſome degrees higher than the former, and in one inſtance ſix degrees, on the firſt day of February, 1776, though the weather was ſerene and the wind at eaſt, when theſe obſervations were taken. Ibid. p. 16. The cold below has been attributed to the froſt that ſtill remained unthawed; but granting that its influence could reach to the height of 20 feet, the queſtion is, whence proceeded the change in the temperature of the upper atmoſphere, which ſome ſhort time before was much colder? It evidently did not proceed from the earth, as it conſtantly does in ſummer.

The

The ſecond remarkable phænomenon is that the North Pacific Ocean, above latitude 40°, is much colder than the North Atlantic, betwixt the ſame parallels. The interior parts of Siberia, eaſt of longitude 100, are much colder than the parts equally diſtant from that meridian on the weſtern ſide. The coaſt and interior of the weſtern regions of America are much colder above latitude 40° than the correſponding tracts of the European continent.

A third ſingular circumſtance is, that barometers, in the northern parts of Europe at leaſt, generally ſtand higheſt in the months of December, January and February. This has been obſerved almoſt conſtantly at Peterſburgh during ten years, (IX Coment. Petrop. p. 325) that is from 1726 to 1736, and during eleven years at Abo, that is from 1750 to 1761. 25 Schwed. Abhand. p. 112; and by Muſchenbr. in Holland, in the year 1728. La Cotte alſo obſerves, that the higher and loweſt ſtates of the barometer occur in the winter months. 44 Roz. Jour. p. 232. It is alſo well known that the ſmalleſt variations occur within the tropics, but gradually increaſe as we recede from them. Ibid.

Now the fact ſtated by Halley, and with which all the above phænomena appear to me to be connected, is, that the equatorial air,

air, and that of the tropic to which the ſun approaches, "being "rarefied by heat and preſſed upon by the colder air, riſes and "diffuſes itſelf above, forming a current in a contrary direction "to the ſubjacent inferior current of the colder air; ſo that a NE. "wind below is attended with a SW. wind above, and a SE. "below with a NW. above." Here his ſtatement ends, but the laſt part of it is erroneous, or at leaſt ambiguous; for from it one would be apt to deduce the exiſtence of two ſimultaneous ſuperior currents, one on the northern and the other on the ſouthern ſide of the Equator, whereas he moſt probably meant two ſucceſſive currents, as he tells us that it is the air of the tropic to which the ſun approaches that flows in a contrary direction to the colder air below; now as the ſun cannot at the ſame time approach both tropics, it follows that theſe currents, like the ſun's approaches, muſt be ſucceſſive; ſo that when the ſun is in, or approaches to the ſouthern tropic, that is, during the winter of the northern hemiſphere, a SE. current prevails in the upper regions of our hemiſphere, and when the ſun, during our ſummer, approaches to, or is in the northern tropic, a NE. wind prevails in the upper regions of the ſouthern hemiſphere; and in fact this equatorial intumeſcence muſt neceſſarily flow and diffuſe itſelf in that direction in which it meets with leaſt reſiſtance, and it meets with leaſt reſiſtance in blowing towards that hemiſphere in which at equal heights the air is moſt rarefied; now when the ſun is in,

or

or approaches to the northern tropic, winter reigns in the ſouthern hemiſphere, therefore the lower ſections of the atmoſpheric columns are more condenſed by cold, and conſequently the upper ſections of thoſe columns are proportionably rarer (ſuppoſing the abſolute weight the ſame) than the correſponding ſections of the northern hemiſphere, where, from the great expanſion occaſioned by the heat below. a greater part of their maſs reaches to the ſame height as the more rarefied part of the ſouthern columns. Therefore during our ſummer, or the winter of the ſouthern hemiſphere, the greater part of the intratropical intumeſcence flows to the *ſouth*. Halley adds alſo to the *eaſt*, in order to preſerve the equilibrium; but this ſeems a miſtake, the equilibrium is ſupported by the inceſſant circumvolving flow below. The direction of the ſuperior current is guided only by the greater or leſſer reſiſtance it meets with; it muſt move, as he himſelf ſays, "*from* thoſe "parts where the greateſt heat is;" and conſequently towards the colder, which at that height muſt be the rarer, and offer leaſt reſiſtance; now the weſtern parts, over which the ſun's influence has not as yet been exerted, are evidently colder than the eaſtern, over which the ſun has already paſſed, therefore the ſuperior current is directed weſtward, or in other words a NE. wind prevails above. The reverſe takes place in the northern hemiſphere during our winter, or in other words a SE wind prevails in the upper regions of our atmoſphere.

AGAIN,

Again, another difference muft be remarked betwixt the direction of the trade winds below and that of the fuperior current. The trade winds are chiefly eafterly, with only a few points to the north or fouth, according to their fituation on the north or fouth fide of the equator, commonly one or two, rarely more. But the direction of the upper current is chiefly to the north or fouth, according to the tropic the fun approaches, with only a few points weftwards, as the greateft cold prevails in the northern or fouthern quarters.

The height above the level of the fea or furface of the earth, at which this intumefcence begins to overflow, is that at which its denfity notably furpaffes that of the aggregate of the adjacent extratropical columns at the fame height, that is about $\frac{1}{35}$ or $\frac{1}{36}$th, and even ftill lefs.

To ftate this point more clearly we muft take a general furvey of the temperatures of the different aggregates of air thus compared, confining ourfelves to the northern hemifphere, as beft known, and to the *winter* feafon.

The

THE equator and tropics lie chiefly over ſea, but partly over land, and the temperatures of each of theſe are very different.

Mean heat of the equatorial air -	84°
——— of the ſupra-marine S. intra-tropical air	85°
——— of ditto incumbent over land -	98°
——— ſupra-marine N. intra-tropical air	80°
——— of ditto incumbent over land -	90°
At the level of the ſea and ſurface of land. mean of all	87°

Mean heat of the extra-tropical ſupra-marine air in this ſeaſon*, from latitude 23° to latitude 33°	66°
Ditto of that incumbent over land -	70°
Mean of both at the level of the ſea and ſurface of the land - - -	68°

To repreſent the action of theſe on each other, we ſhall ſuppoſe each to form a diſtinct column, and both columns to be contiguous to each other, and each to ſupport mercury in the barometer to the height of 30 inches, at the ſurface of the earth.

* See Eſtimate of the temperatures of different latitudes.

And here it is plain in the firft place, that, as both are of equal weight, the intra-tropical column, being more expanded by heat, muft reach to a greater height than the colder and lefs expanded extra-tropical air. But that at a certain height the weight and denfity of the intra-tropical air muft be confiderably greater, and confequently that this air muft flow over or into the other, will now appear by fhewing the elevation that mercury would ftand at in each, at that given height. The height I now allude to is that of the upper line of congelation; in the mean of the above-mentioned latitudes of the *extra-tropical* air, this height is 21800 feet = 3633 fathoms, which, fubtracted from log. 30 (= 4771212) gives the logarithmic number 1138212; and this correfponds with the natural number 12,997, I. to this height, the mercury would then rife in the barometer. But in the column reprefenting the *intra-tropical* air, the mercury would rife at the fame elevation over the earth to 13,835 inches, as appears by the following calculation. The mean height of the line of congelation of the intra-tropical air is 25000 feet, and the mean heat of this air, at the furface of the earth being 87°, the difference of this with 32° is 55°, which divided by 250,00 quotes 0,222 the common difference of the progreffion; and this multiplied into 21800 (218,00 being expreffed in the terms of the progreffion) gives the diminution of heat

heat 48°,396, which, fubtracted from 87° gives the temperature at that height, 38,664. The mean heat of this column is therefore 62°,8, which exceeds the temperature of 32° by 30°,8. Now, according to Sir George Schuckburgh's calculation, 1000 feet of air, by one degree of heat above 32°, gain 2,43 feet, confequently 21800 feet of air fhould gain 52,97 feet. And if one degree of heat gives an increafe of 52,97 feet, 30,8 degrees fhould give 1631,476 feet; confequently the mercury fhould rife in it to the fame elevation as if the barometer had been placed 1631,476 lower, that is at the height of 21800 — 1631,476 = 20168,524 feet = 3361,420 fathom, which fubtracted from log. 30, as above, gives the logarithmic number 1409792, correfponding with the natural number 13,835. At this number of inches then the mercury would ftand in the intr-atropical air. The denfity of the fuperior air incumbent upon it exceeds that of the air incumbent over the extra-tropical air of the fame height over the earth by ,838 of an inch of mercury, or about $\frac{1}{36}$ of the whole weight of each column. It muft therefore flow over, or into the rarer extra-tropical air.

This overflow takes place at even far lower heights in the atmofphere; for by a fimilar calculation it will be found that the denfity of the intra-tropical air exceeds that of the extra-tropical by $\frac{3}{10}$ of an inch, even at the height of 8000 feet.

THIS ſuperior current is inceſſantly propelled forward by the inceſſant ſucceſſion and propulſion from behind; and its rapidity increaſed proportionally to the decreaſed denſity of the more northern columns to which it proceeds.

THE heat it poſſeſſes at the height of 21800 feet was given at the average both of the ſupra-marine and ſupra-terrene columns; but it is evident that the heat of the ſupra-marine columns at that height is ſome degrees lower, and that of the ſupra-terrene ſome degrees higher. At the height of 1000 feet the heat of the ſupra-marine intra-tropical air is 57°,6, and of the ſupra-terrene 70°,6.

Sir Charles Blagden, in a very intereſting paper inſerted in the Philoſophical Tranſ. 1781, p. 341, has ſhewn that the Gulf ſtream, paſſing northwards from the Gulf of Mexico through water ſeveral degrees colder, loſes only two degrees of heat for every three degrees of latitude it paſſes through; and Dr. Franklin, in a paper of four years later date, in the ſecond volume of the American Tranſ. p. 316, informs us that this ſtream preſerves its ſuperiority of temperature at leaſt from latitude 25° to latitude 44°, which it reaches in twenty or thirty days, that is through 19 degrees of latitude about 1300 miles; but Sir Benjamin Thompſon (now Count Rumford) in the Phil. Tranſ. of 1786, p. 304, has ſhewn that atmoſpheric air is four times a worſe conductor of heat than water

water is, and confequently parts with it more flowly in that proportion. Therefore it lofes only 2° of heat in paffing through 12° of latitude, and 6° degrees in paffing through 24° of latitude, and 8° in paffing through 48°. Therefore the *fupra-terrene* intra-tropical air at the height of 10000 feet is cooled down in latitude 72°, that is 48° beyond the northern tropic, only 8°, and confequently its temperature, even in that diftant latitude, is 64°. But the *fupra-marine* intra-tropical air, whofe temperature at the height of 10000 feet is 57°, is cooled down to 49° at latitude 72°. It is true, that the degree of moifture, in the air through which the intratropical air paffes, muft occafion a confiderable difference as to the heat it retains, for Count Rumford has alfo fhewn, that moift air is 3,9 times a better conductor of heat than common air, and confequently nearly as good a conductor as water. Ibid. But the air, in his experiments, was faturated with moifture, a circumftance that cannot be fuppofed in the cafes at prefent referred to. From thefe principles the explication of the above-mentioned phænomena is clearly deduced

For 1°. the fuperior ftrata of the atmofphere are obvioufly warmer than the lower, being occupied by the fuperior current, whofe heat is gradually communicated to the lower ftrata, until at length it becomes uniform, as in the cafes obferved by De Luc.

2do.

2d°. The North Pacific Ocean is colder than the North Atlantic between the fame parallels, becaufe the fuperior current that paffes over the North Pacific is entirely *fupra-marine*, and for the fame reafon eaftern Siberia is much colder than its more weftern tracts. But the current that paffes over the eaftern parts of the North Atlantic is in great meafure *fupra-terrene*, for it iffues from Guinea, Senegambia, and a fkirt of the great defert. So alfo the current that paffes over the weftern parts of Siberia, being derived from Siam, Ava, and the more fouthern iflands. But the current that paffes over the eaftern regions of North America is entirely *fupra-marine*, as it originates on the Atlantic Ocean; whereas that which paffes over the correfponding European tracts, Hungary, Poland, Germany, Sweden, France, Spain, Italy, and the Britifh iflands, is entirely *fupra-terrene*, arifing from air fuper-incumbent on fouthern and northern Africa. Thefe directions from the fouth to the N. N. Weft may eafily be traced on a map, obferving to allow from one to three points to the weftern direction.

The 3d phœnomenon is due to the re-inforcement of the fame caufe. In the months of December, January, and February, the fuperior current is then more copious, as the intra-tropical air is is then more heated, and hence adds more to the weight of the northern air, and confequently mercury in barometers muft ftand higheft; but as this current foon diffufes itfelf over regions

on

on which air at an equal height is ſtill more rarefied, the mercury muſt ſink in proportion to the quantity of air that deſerts its ſtation. The ſmall variations of mercury in the intra-tropical regions proceed from the ſmall alteration of the quantity of air incumbent over thoſe tracts. The perturbations that take place within the tropics originate in far higher ſtrata of the atmoſphere, than thoſe do that take place in the more diſtant extra-tropical tracts. Thus, Gentil has ſhewn, that thunder proceeded from from clouds 10000 feet above the ſurface of the earth at Pondicherry, latitude 12°. 2 Gentil, p. 79. But in latitude 46°, Sir George Schuckburgh heard thunder grumbling under him when ſtanding on mount Saleve, an elevation of only 2831 feet over the ſurface of the plains. Phil. Tranſ. 1777, p. 527. Now as a great part of the weight of the atmoſphere reſides in the inferior and denſer ſtrata, it is evident that it muſt be more altered by the perturbations that happen in them, than by thoſe that happen in the much loftier. Hurricanes alone affect the lower ſtrata, and hence the barometer ſinks conſiderably. Thus, in the hurricane that happened in the iſland of St. Bartholemew on the 2d of Auguſt, 1792, the barometer fell from 30,18 to 28,03 during its continuance, and perhaps ſtill lower, for the obſerver was obliged to quit the houſe, whoſe proſtration he apprehended, when at its height. See the circumſtantial deſcription in XI. Voights's Phy. Magaz. 4 Stuck. p. 74.

It

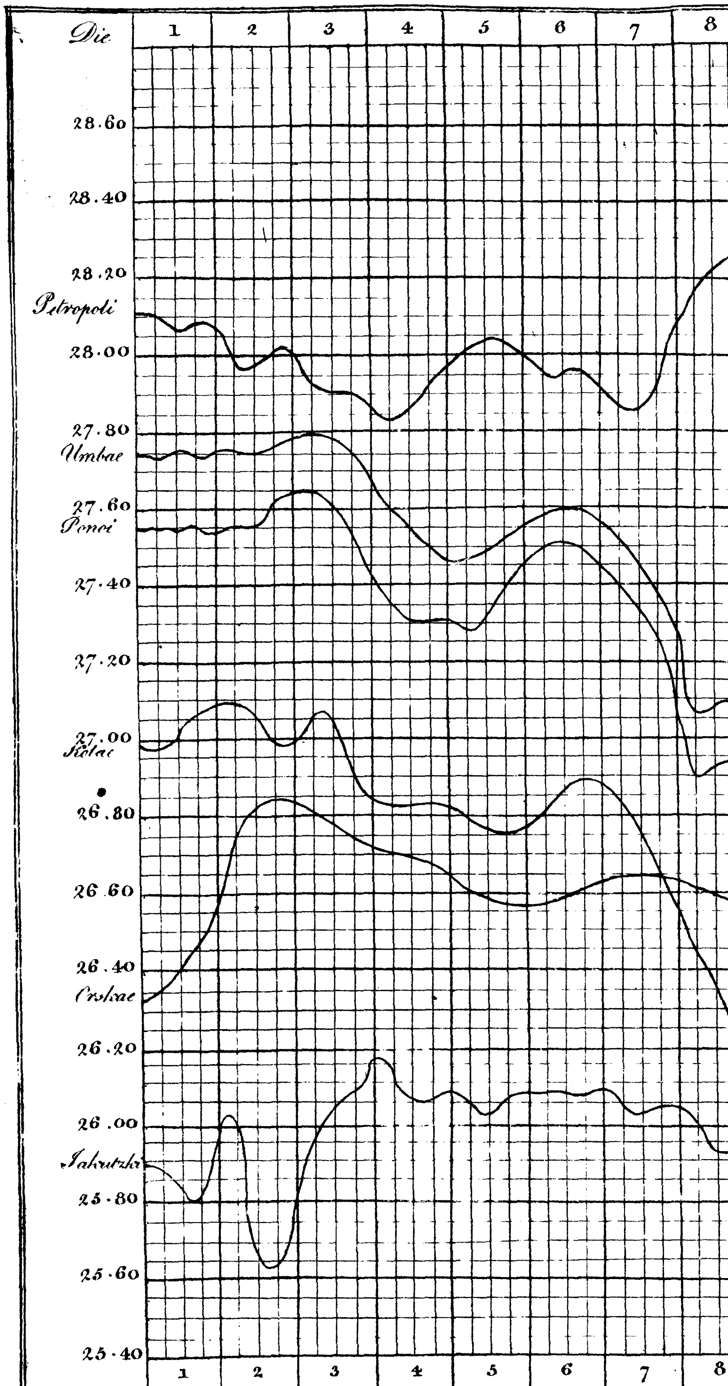
It may be objected, that mercury rises to great heights above its natural state even in summer, when no superior tropical effluence prevails, which is certainly true; but it must be remarked, that a vast accession of air arises in that season from the decomposition of water in growing vegetables, as Dr. Hales has abundantly proved, and is now universally acknowledged. Nay, Melander Hielm informs us, that the quantity of air emitted from gun-powder during a battle raises mercury in barometers situated in the vicinity. 5 Nev. Schwed. Abhandl. 1784, p. 9. Nor should we suspect that the atmosphere should thus receive an annual increase, as much is destroyed by volcanos, tornadoes, hurricanes, and putrefying substances, &c.

Most of the particulars I have advanced, relative to the influence and difference of the superior currents, receive the fullest confirmation from Euler's synoptical table of the variations of the barometer during the month of May, 1769, in the vast empire of Russia, including an extent of observation, amounting to nearly 4000 miles, from the western parts of Russian Lapland to Iakutz in Siberia, and eleven degrees from north to south. The places where the cotemporaneous states of the barometer were observed were*:

* *This table I have here copied from the 14th vol. of the N. Acta Petropol.*

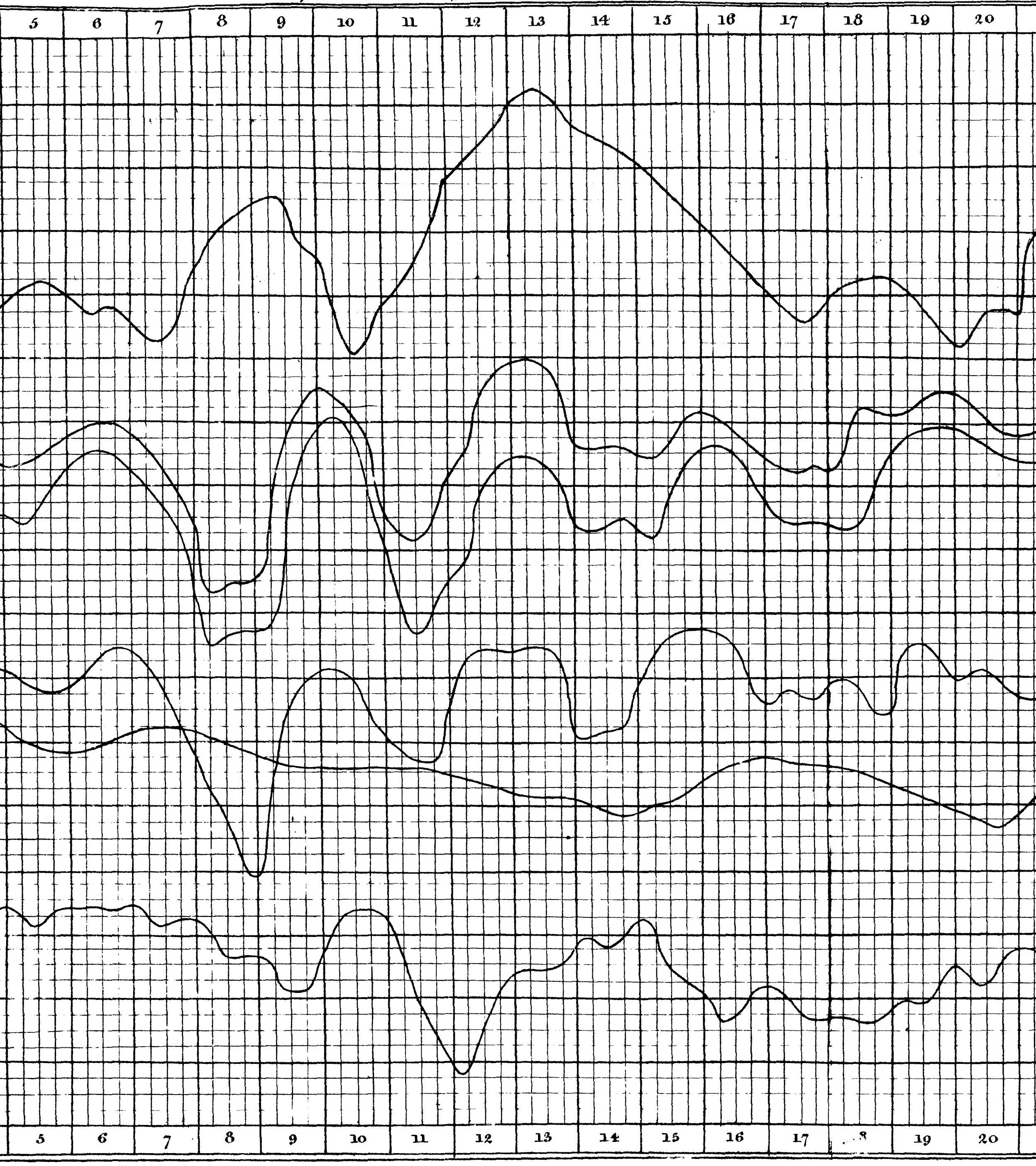
Latitude.

Altitudines Ba



Altitudines Barometricae mense Maji (A
quarum abscissae temporibus Observationum
tortuosis nimium nectantur; et quo melius, ea a
ita ut pro oppido Ponoi 20 partes centesimae, p
hic notatas.

Altitudines Barometri per mensem Maji Anni MDCCLXIX Petropoli, Kolae, Ponoi, Umbae, Ors



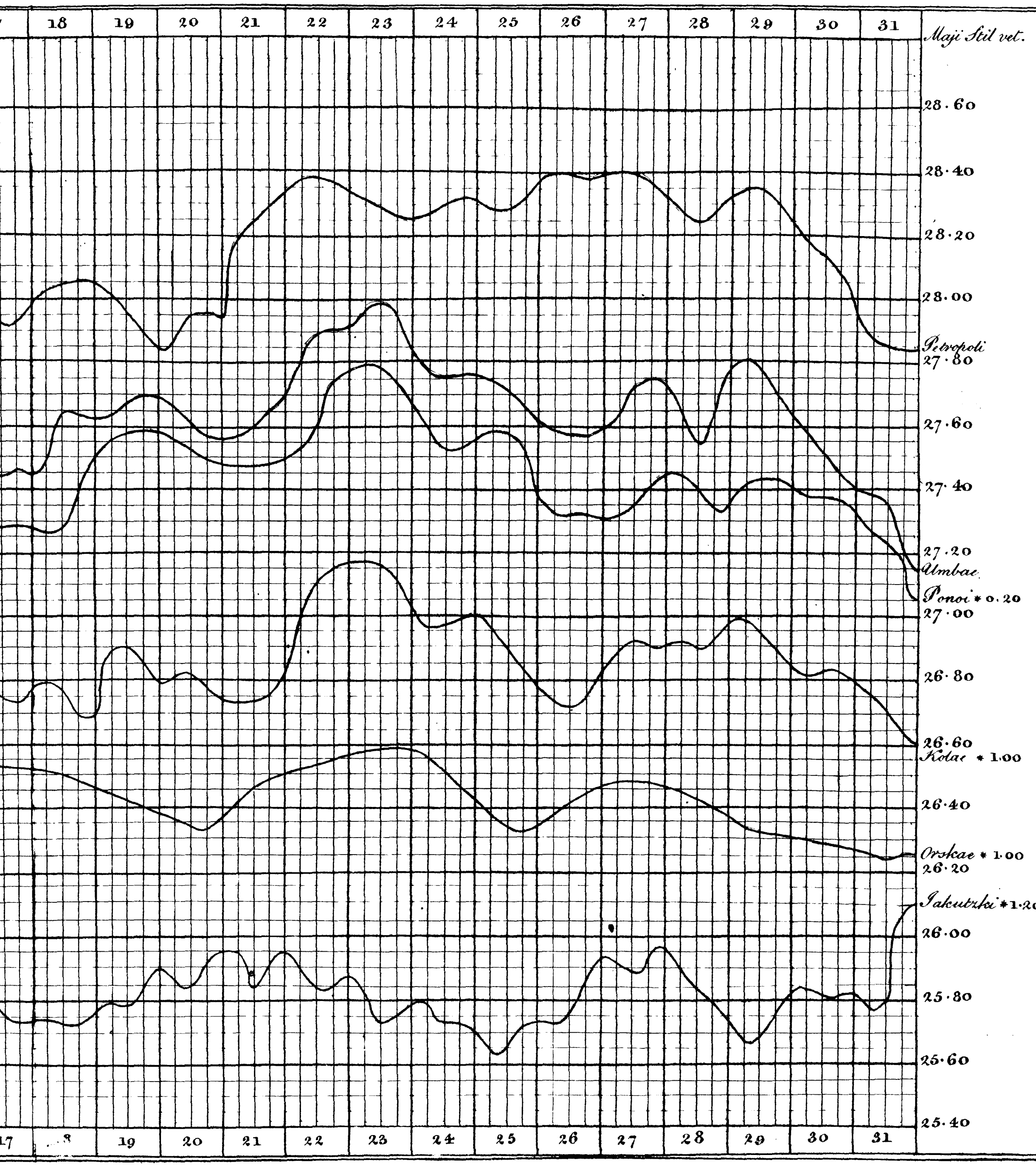
Explicatio Figurae

icae mense Maji Anni 1769 Petropoli Kolae Ponoi Umbae Orskae et Iakutzki st

oribus Observationum, ad Meridianum Petropolitanum reductis, et applicatae Altitudinibu

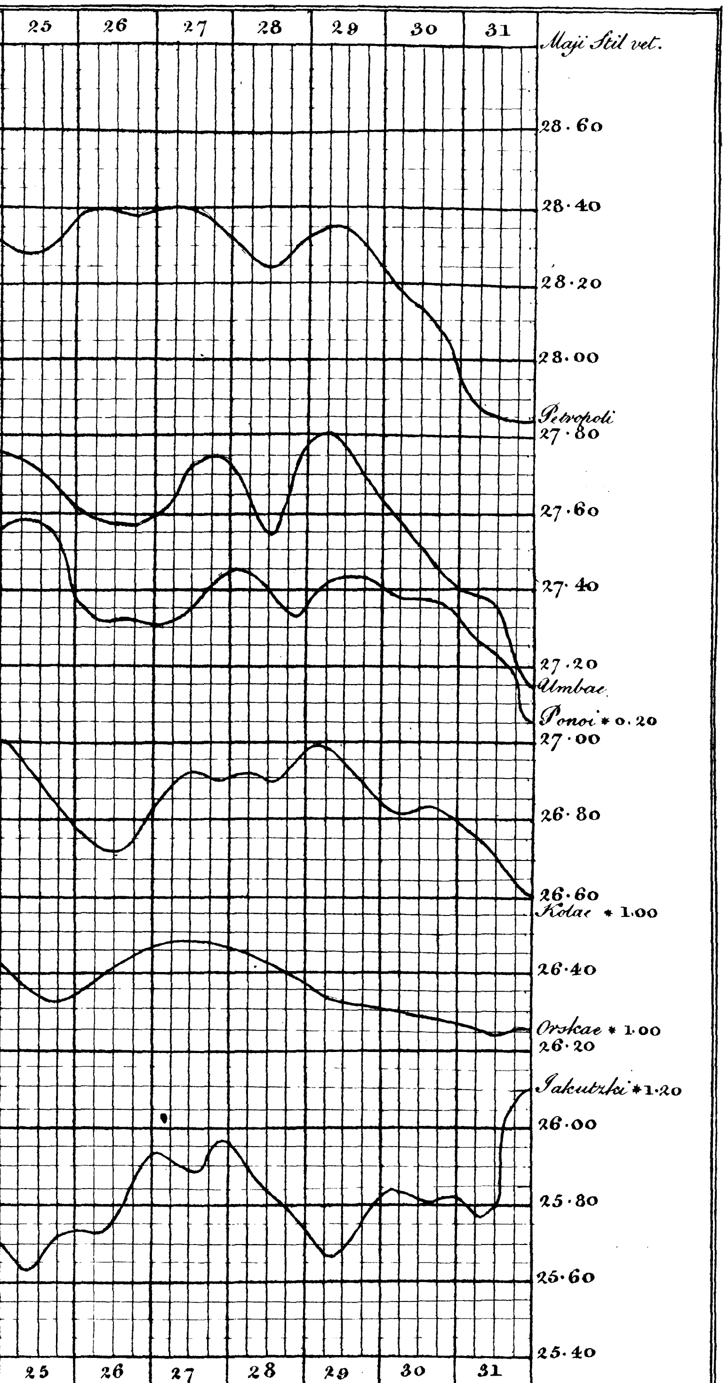
ur, et quo melius eae a se invicem distingui queant, minores aliquantum reddidi vel depre

20 partes centisimae, pro Kola et Orsk vero 100 ejusmodi partes. et pro Iakutzk adeo 120 partes cen



urae

kae et Jakutzki st: vet: Observatae in hac Tabula per lineas curvas repraesentantur.
plicatae Altitudinibus Barom: ipsis respondent. Ne autem hae curvae flexibus earum
m reddidi vel depressi altitudines Barometri Ponoi, Kolae Orskae et Jakutzki Observatae
utzk adeo 120 partes centesimae pollicis parisini addendae Sint ad Altitudines Barometricas



hac Tabula per lineas curvas repraesentantur.
spondent. Ne autem hae curvae flexibus earum
ometri Ponoi, Kolae Orskae et Jakutzki Observatas
ni addendae. Sint ad Altitudines Barometricas

	Latitude.	Longitude.
Kola - -	68°,52′ - -	30°
Ponoi - -	67° - -	35°
Umba - -	66° - -	37°
Peterſburgh -	59°,56′ - -	30°
Iakutz - -	62° - -	102°
Orſka - -	51° - -	60°

On examining this table it will be ſeen, 1°. That the mercury riſes *ſooner* in the *ſouthern latitudes* than in the *northern*, (when both are ſubject to the ſame current) thus it was riſing on the *firſt* of May at Orſka while it was falling at Peterſburgh. See alſo May 7th, 15th, and 20th. And it began to riſe ſooner at Peterſburgh than at Ponoi or Umba. See May 4th, 8th, and 11th. But the variations of the riſe and fall are leſs diſtant in point of time, becauſe, though Kola is more northern than Peterſburgh, it is alſo more eaſtern, and therefore the appulſe of the current is more nearly ſimultaneous than its appulſe to Orſka, which is both more ſouthern and more eaſtern, and therefore receives the ſuperior effluence ſooner.

2°. Ponoi and Umba are near each other, and therefore their variations, in every ſenſe, differ little from each other; but Umba being ſome degrees more caſterly, the riſe there ſomewhat precedes that at Ponoi.

3tio. The variations at Iakutz, proceeding from a different effluence, namely, the *fupra-marine*, have no connexion with the preceding; this the table fufficiently indicates in every inftance, but in none fo remarkably as on the 31ft of May, for while the mercury in all the other above-mentioned places is on the fall, (which proves the identity of the influence to which they are fubjected) it rifes at Iakutz.

This fynoptical view of barometrical obfervations at different diftant places is the only one, I believe, as yet exhibited; yet if it had been extended to latitudes ftill more fouthern, as it eafily might, much light could not fail of being thrown on this obfcure fubject.

Section III.

Of the Origin of the General Trade Winds.

Though the origin of the general trade winds appears to me to have been fully eftablifhed by Dr. Halley, yet it feems he has explained himfelf too briefly, fince his explanation has been mifunderftood by many, and was thought obfcure even by D'Alembert*.

* Sur la Caufe des Vents. V.

To

To underſtand it more perfectly, let us ſuppoſe the ſun for the firſt time in the meridian, and to communicate its heat every inſtant fifteen degrees all around. If it were to remain in this ſituation the ſurrounding air could have no other motion but upwards, for the lateral dilatations being equal, would neceſſarily check each other, but in the ſecond, and all the ſucceeding inſtants, the ſun moves weſtwards; therefore, of the originally equidiſtant eaſtern and weſtern points, the weſtern, to which the ſun approaches nearer, is more heated than the eaſtern, from which the ſun recedes; therefore, in this, and all the ſucceeding inſtants, the eaſtern, being more cooled, will preſs on the weſtern, and thus an eaſtern wind will be eſtabliſhed.

It is true, that, in the northern hemiſphere, the northern air alſo preſſes upon the more heated ſpaces, but as this alſo follows the ſun's path to the weſtward, it becomes alſo eaſterly, preſerving only a few points of its primitive direction. D'Alembert adds alſo the ſolar attraction, which, according to him, elevates the air in the points over which the ſun is vertical, and, conſequently, produces a dilatation advancing from eaſt to weſt. But Mr. De la Place, not denying this cauſe, conſiders it too weak to produce ſingly any conſiderable effect*.

* Mem. Paris, 1776.

About the year 1735 Mr. Hadley publiſhed a very different account of the origin of the trade winds. 8 Phil. Tranſ. Abrid. p. 500, which, however, has been rejected by the moſt diſtinguiſhed aſtronomers that have ſince attended to this object, as D'Alembert, ſur la Cauſe des Vents. Art. 376, and 385, Gentil Voy. 2 Bergman Erde Beſchreib. p. 91.

According to Mr. Hadley, the air, being rarefied towards the equator, is conſequently invaded, in the northern hemiſphere by the northern, and in the ſouthern hemiſphere by the ſouthern colder air.

But as the parallels of latitude enlarge as they approach the equator, and as the equatorial ſpace is nearly in the proportion of 1000 to 917, the difference of their circumference is nearly 2083 miles; conſequently, the ſurface of the globe at the equator moves ſo much faſter than under the tropics; and hence the northern or ſouthern air, moving from the tropics towards the equator, muſt poſſeſs leſs velocity than the parts it arrives at, and, conſequently, appear to move in a direction contrary to that of the earth's motion, which being from weſt to eaſt, the air arriving ſooner at the weſtern parts, will appear to move from eaſt to weſt, and this relative motion being combined with that towards the equator, a north-eaſt wind will be produced on the north ſide, and a ſouth-

eaſt

eaſt wind on the ſouth ſide of the equator.—Theſe, as they approach the equator, ſhould become ſtronger and more eaſterly, and appear due eaſt in the equator itſelf, by reaſon of the concourſe of both currents from the north and from the ſouth.—There the velocity of each ſhould be at the rate of 2083 miles in the ſpace of one natural day, or above 1,33 miles per minute, if it had not been that before the air at the tropics could arrive at the equator, it muſt have gained ſome motion eaſtwards from the ſurface of the earth or ſea, whereby the relative motion is diminiſhed to the degree that actually exiſts in it.

THIS theory appears to me rather ingenious than ſolid, for the following reaſons:

1°. THE trade winds are commonly gentle, moving only at the rate of eight miles an hour; therefore they have ſufficient time to gain or participate of the motion of the earth; therefore their contrary courſe muſt ariſe from an abſolute cauſe, and cannot be deemed merely relative

2°. BECAUSE the north-eaſt wind ſcarce ever approaches nearer than eight or ten degrees to the equator, and there dies away; whereas it ought there, according to this theory, to be ſtrongeſt. And, on the contrary, the ſouth-eaſt paſſes the equator ſeveral degrees,

grees even when the ſun is in the ſouth tropic. A fact which as Gentil remarks is abſolutely irrreconcilable with this theory. 1 Gentil Voy. p. 638, 5 Gentil p. 116.

3tio. BECAUSE if the conſtant eaſterly wind was in the northern hemiſphere ſupplied ſolely from the north, and in the ſouthern hemiſphere ſolely from the ſouth, we ſhould in the former have a conſtant north wind at leaſt at 35 or 40 degrees from the equator, or at leaſt from ſome northern point, and in the latter a conſtant ſouth wind, or at leaſt from ſome ſouthern point; whereas on the contrary a ſouth wind often prevails in thoſe latitudes on the north ſide of the equator, and a north wind on the ſouth ſide.—Thus La Peyrouſe met an E. S. E. in north latitude 32°, and a due eaſt in latitude 31°, and a S. S. E. in latitude 14°, and a due eaſt in latitude 16°, and a due north in latitude 20°, (where then was the relative motion?) and a due ſouth in latitude 33°. See his Journal in 3d La Peyr. Voy. He alſo met with a due north in latitude 27° and 42° ſouth, and a N. N. E. in latitude 25° ſouth. So Captain Cook met a S. S. E. wind in latitude 30° north, and alſo in latitudes 40° and 41°, and a due ſouth wind in latitude 38° and 20°; and in the ſouthern hemiſphere a due north in latitude 3°, 4°, and 44°. I might produce other inſtances from ſea journals, and particularly from that moſt ample and inſtructive, kept by Major Dalrymple during a voyage to the Eaſt Indies. Phil. Tranſ. 1778, but

but I think the alleged ſufficiently prove that the general eaſt wind is not ſupplied ſolely from the north or ſouth in the different hemiſpheres reſpectively.

4°. Because during our ſix ſummer months, when the ſun is in or approaches to the northern tropic, the eaſterly trade wind partakes leſs of the northerly, than when the ſun is in or approaches to the ſouthern tropic. 2d. Phil. Tranſ. Abrid. p. 134, and Schued. Abhandl. 1762, p. 175, which is directly contrary to Hadley's ſyſtem; for when the ſun is in the ſouthern tropic, the north wind muſt traverſe more of that ſpace in which the earth's motion eaſtward is ſtrongeſt, and therefore ſhould participate more of that motion as Hadley himſelf ſtates: though ſtill partaking of it in a ſmaller degree than that which the globe itſelf poſſeſſes, it ſhould appear to move weſtwards; yet it ſhould proportionably retain leſs of its original direction from north to ſouth, than when it had traverſed a ſpace more diſtant from the equator, whereas the fact is, that it retains more, and often paſſes into the ſouthern hemiſphere into the 13° ſouth latitude without having any eaſtern direction. 3 Marchand, p. 551; and an analogous fact is obſerved with reſpect to the ſouth eaſt wind when the ſun is in the northern tropic. Hence it is evident, that it is from the approach of the ſun, and not from the latitude traverſed, that the eaſtern direction is derived; nay the wind is often more eaſterly than northerly between latitude 23° and

and 28, Foſter's Obſervations p. 126. He even obſerved, that the trade winds extended far beyond the tropics when the ſun is in the ſame hemiſphere, which ſhews it is the ſun that cauſes them.

EDDY is a term inttoduced on this ſubject, which explains nothing when its cauſe is not aſſigned and proved; the trade winds are often interrupted by the approach of land, but the interruption, as Foſter mentions, extends only to a few miles. Ibid. 127.

THE *monſoons* or periodical trade winds, depending on local circumſtances ſufficiently explained by Dr. Halley, I ſhall here paſs over; though certainly much may be added from obſervations made by ſubſequent navigators and travellers. I ſhall therefore confine myſelf to the *variable* winds, a ſubject much more obſcure.

SECTION IV.

Of Variable Winds.

WITH reſpect to winds we muſt lay down one general and fundamental principle, which is, that they always originate at the extremity of that point towards which they proceed. Thus the

the eafterly trade wind begins at the point neareft the fun, which it follows, and is perpetually renovated and fupplied from parts ftill more eafterly.—Thus in the year 1709 a north wind was fooner perceived in England than at Dantzick. 4 Phil. Tranf. Abrid. 2d part, p. 115. And Wargentin notes that when the wind changes to the weft, this change takes place at Mofcow before it happens at Abo, which is feveral degrees weft of it, and fooner in Finland than in Sweden. Schwd. Abhandl. 1762, p. 195. And Dr. Franklin, in his XXXVI. Letter, p. 389, thinks that the north-eaft ftorms in North America begin firft in point of time in the S. W. parts; that is to fay, fooner in Georgia than in Carolina, and fooner in Carolina than in Virginia, &c. He found that a north-eaft ftorm began at Philadelphia at feven o'clock, but did not extend to Bofton (about forty miles to the north-eaft) until eleven o'clock. The reafon of which he well explains, as the current muft begin in the places neareft to that in which the rarefaction arifes towards which the current is directed.

Of Wefterly Winds.

THAT eminent and laborious meteorologift, citizen La Cotte, infers from numerous obfervations of many years, that between

latitude 47° and 60° on the weftern fide of our hemifphere, the weft wind, with fome participation of the north or fouth, is that which obtains ofteneft. 39 Roz. Jour. p. 267. Leche obtained the fame refult at Abo, latitude 60°, from twelve years obfervations, Mufchenbroock in Utrecht, Mr. Dalton in Weftmorland, latitude 54° (fee his Meteorolog. Effays, p. 48 and 88) from five years obfervations.

THIS wind in our continent originates in the Pacific ocean, between the above mentioned parallels, at leaft in winter; the air incumbent on that ocean is then much warmer than that of Siberia and Chinefe Tartary that lie weft of it, this therefore preffes upon and flows into the fupra-marine, and is immediately fucceeded by air ftill farther weftwards, and thus a current is gradually eftablifhed extending to the Atlantic, which, though in winter, being much warmer than the air of the iflands and continent on which it flows, is forced into the current, both by the rupture of the equilibrium to the eaftwards, and by the preffure of the much colder air of the continent of North America.

Of

Of Easterly Winds.

During the winter months there seems to be a frequent struggle and contest betwixt the air incumbent over the Asiatic continent, and that incumbent on the North American, lying betwixt the above-mentioned parallels and bordering on the Pacific ocean, which of them shall rule over it.

The mass of the American air being less considerable, and its efforts divided between the Pacific and the Atlantic, is generally obliged to yield to its antagonist; though sometimes the Asiatic being warmed, either by a diffusion of the superior current or by southerly winds, the colder American becomes more forcible. In summer this must happen frequently, the E. N. E. oftenest prevailing; upon the whole however Leche remarked that the E. and E. S. E. were nearly the most uncommon; as did La Cotte in the climate of Paris. Meteorolog. p. 305.

With us this wind is most frequent in the months of April and May; and I have observed in Cook's Journal, tables 9th, 10th and 11th, that it prevails also in the same months in the Pacific, therefore the colder continental air then pours in upon us.

La Cotte alſo obſerves, that in the weſtern tracts of Europe, in latitudes below 48°, this wind occurs ofteneſt during the winter months*, for the ſuperior heat of the Atlantic in the low latitudes determines the colder air incumbent on Hungary and European Turkey to flow in upon it.

Of Southerly Winds.

A few years ago no problem in meteorology appeared to me more difficult than to aſſign a cauſe for the frequent prevalence of a ſouth wind even in winter, it being contrary to the laws of na-nature that warm air ſhould ruſh upon colder, yet I ſince diſcovered, that the conjectural ſolution I then offered is grounded on a real fact.

In the eaſtern parts of our hemiſphere, from longitude 72° to 160°, that is, from the coaſt of Malabar to the Moluccas, it blows from the north-eaſt conſtantly from October until April. Now this northern blaſt muſt be ſupplied and recruited from countries ſtill farther north, until we arrive at the pole, and the polar air muſt conſequently be ſupplied by that which lies ſouth of it, and

* 2 Mem. Meteorolog. p. 189, &c.

thus

thus a southern current is established on the western side of our hemisphere.

INSTANCES to support or contradict this theory do not often occur, yet I have found some that appear to me decisive, independently of the general reason alleged. Thus I find in the 9th table of the third vol. of Cook's Voy. that in north latitude 59°, and east longitude 207, on the 25th of May, 1778, a strong north-west wind prevailed; and on the 29th day of the same month and and year, an equally strong S. W. wind prevailed at Petersburgh, latitude 60°, and longitude 30 E. Now the places of observation were 177 degrees distant, one on the eastern and the other on the western side of our hemisphere, (which, at this proximity to the pole, argues not, a superior distance to that I have mentioned) and four days is as short a time as can be allowed to the S. W. to supply the more eastern N. W. Mem. Petersburgh, 1778, p. 92. So also in the same journal I find, that from the 4th to the 30th of May, a north wind prevailed in the eastern part of our hemisphere from latitude 58° to 61°, except seventeen days of variable winds; but in London it blew from the S. W. during the first fifteen days of June, thus replacing the northern air. And to replace the constant N. E. wind, on the Indian Peninsula to the Moluccas, there is a constant draught from the south in the western parts of our hemisphere; accordingly Leske observed, that on an average of

of twelve years, it blew 126 days each year, from October until May, from some south point, namely, 86 days from the south or southwest, and 40 from the south-east, at Abo, lat. 60°.

It is true, that he found it to take place very frequently also in summer, but this is occasioned by the great heat that then prevails in the northern tracts of Lapland.

And, upon the whole, more of the south air is drawn off in winter than in summer, for its flow is gentle in summer, but often stormy in winter. See Leske's IX. XI. and XII. tables. If all other meteorological tables of a series of years had been arranged with equal sagacity and precision as those of Leche and Dr. Horseley, a vast fund of information might be extracted from them.

At Petersburgh, during the year 1793, Euler junior found a south or south-west wind prevailed 79 days, 52 from October to the end of March, and only 27 in the summer months; it was stormy in November, December, and January. I have not noted the S. E.

Mr. Stritter also found the south wind to predominate at Moscow during the six winter months of that year. 11 N. Acta Petrop. p. 569. So that the frequency of this wind in high latitudes is certain.

Of

Of Northerly Winds.

In the weſtern parts of our continent and hemiſphere theſe are of all others the leaſt frequent in latitudes above 48°. See La Cotte and Leche's tables. The cauſe of this unfrequency appears from what has been ſaid of ſouth winds.

But in latitudes below 48° they occur oftener, and ofteneſt in thoſe that are ſtill lower, as La Cotte remarks. An admirable inſtance of Divine Providence, that the warmeſt winds ſhould prevail ofteneſt in winter in the coldeſt regions, and cold winds in the warmeſt!

But it may be aſked, why a ſouth wind ſhould not prevail in the eaſtern parts of our hemiſphere to ſupply the conſtant N. E. wind that prevails in the low latitudes of the weſtern ſide? The reaſon is, that on the weſtern ſide the N. E. winds of low latitudes are eaſily ſupplied by the contiguous Atlantic, which is open up to the North Pole; and, as here, the upper current ſets and ceaſes, there can be no deficiency of air.

Of

Of oppofite Concomitant Winds.

It has often been obferved*, but of late, fince the invention of baloons, evidently proved, that currents of air from different and even oppofite points of the horizon, prevail at different heights in the atmofphere, over the fame tracts of land or water. This was originally inferred from the different courfes of the higher and lower clouds; but as fuch obfervations were often liable to optical deceptions, better proofs were wanting.

Mont Louis is within thirty miles of Perpignan, but about 5000 feet higher. Now in March 1780 north and north-eaft winds prevailed at Perpignan and a wefterly wind at St. Louis. In Auguft a north wind prevailed at Perpignan and an eaft at Mont Louis. Mem. De La Société de Medecine de Paris, 1780. Derham fufpected†, and Gentil has fince fhewn, that changes of feafons conftantly begin in the upper atmofphere; while a ftrong wind blows from one point below, a wind from an oppofite point reigns above, but more gentle, until at laft (in about three weeks) it is propagated downwards. 2 Voy. p. 23 and 24, in 8vo. The lower atmofphere, he fays, extends to the height of 2880 feet.

4 vol.

* 2 Ulloa's Voy. p. 62, Englifh.

† 4 Phil. Tranf. Abridg. 2 Part. 125.

4 vol. p. 48. At the commencement of winter, when the ſun approaches the ſouth tropic, and the north air begins to flow in and follow it, it muſt meet with more reſiſtance from the lower denſer air, as its impetuous courſe in an oppoſite direction is more ſlowly altered (this reſpects the monſoons) than in the rarer ſuperior ſtrata; and the ſame effect, but in a different direction, takes place when the ſun approaches to the northern tropic.

It has been ſaid by many, that winds in the ſuperior regions of the atmoſphere are much more violent and impetuous than in the lower. Sauſſure Hygrom. p. 300. 2 Ulloa's Voy. p. 81. Muſchenbr. § 2612. 2 Bergm. Erde kugel. p. 99. De Luc, &c. But the contrary has alſo been obſerved by Gentil, above quoted, and Morveau. Aeroſt. de Dijon.

Of the Succeſſion of Winds.

Well eſtabliſhed general laws on this head would be extremely uſeful, as we might then foreſee what wind might next be expected. Beſides the *general* ſucceſſion in an open country, it is probable there is a *local*, confined to certain ſituations.

Gentil remarks, that in the ſouthern latitudes of our hemiſphere, a north-eaſt is ſucceeded by an eaſt, ſouth-eaſt, and ſouth.

According to La Cotte, the order of fucceffion in the middle latitudes is fouth-weft, north, weft, north-eaft, fouth, north-weft, eaft, fouth-caft. 39 Roz. Journ. p. 267.

Of the Scirocco.

THIS is a fouth, or fouth-eaft wind, known in the fouthern parts of Italy, Sicily and Malta, diftinguifhed by peculiar debilitating effects, well defcribed by Brydone, and by Dolomieu in his treatife on the temperature of Malta. The latter has fhewn that its malignity refults from the conftitution of the air it conveys, and not merely from its temperature, which is variable, from 55° to 80°. It contains a much fmaller proportion of oxygen than air ufually does. The conftitution of the African wind, called Harmattan, is as yet unknown; it is, at leaft on land, loaded with fome unknown undiffolved vapour, and is much hotter and drier than the Scirocco, but not debilitating, and even wholefome for animals; for though it parches their fkin it deftroys infection and cures feveral diforders. See Phil. Tranf. 1781, p. 46, &c. Its direction is alfo weftwards.

SECTION

SECTION VI.

Of the Variations of the Temperature of the Summer and Winter Seafons that take place in different Years.

To reafon with precifion on this fubject, we muft at firft abftract from all fublunary phyfical caufes, and indicate the temperature appropriated to different latitudes from mere aftronomical confiderations.

HALLEY has ingenioufly refolved this problem fo far as the mere ratios of heat in the different feafons are concerned. 2 Phil. Tranf. Abr, p. 165. And Lambert in his Pyrometric, § 596.

HALLEY, calculating the *ratios* of heat communicated by the fun to the earth, (which he confiders merely as a planet, abftracting from all diftinction of land and water) in the different feafons in the northern hemifphere, reduces thefe feafons to three, the equinoxes, the fummer folftice, and the winter folftice; and attending only to the fines of incidence of the fun's rays, and the duration of their action, he fets the heat communicated at latitude 0. On the days of the vernal and autumnal equinoxes at 20000. And on the tropical days in the fame latitude at 18341. And then adds the ratios which the heat in every 10th degree of north latitude bears to thefe at the fame periods. Lambert adds the ratios of lat. 49° and 66°,33', ftating the equatorial heat on the equinoxial day at 999.

But to expreſs theſe ratios in thermometrical meaſures we muſt endeavour to find the greateſt heat of the equinoxial day, taking a mean of the heat of the morning at two o'clock, and the evening, under the equator, or very near it; and this I find to be 88° or 89° of Fahr. (See Ulloa, Mem Philoſoph. p. 61) in the northern hemiſphere, on the 20th of March, on the ocean, to which indeed we muſt confine ourſelves in this enquiry, and particularly the Atlantic, for no uniformity can be expected on land.

It is uncertain what thermometer of Reaumur Ulloa employed, whether the true or the falſe, and hence I place the heat at 88° of Fahr.

This correſpondence being found, the thermometrical degrees correſponding with all the other ratios are eaſily found by the rule of proportion, and the degrees thus found I call the *mathematical temperature*. But in moſt caſes this temperature is far from agreeing with the temperature really obſerved; and which I therefore call the *real* temperature; this I take at a *mean*, and not at its *maximum*, which I could not always diſcover, and is more fugitive and contingent. Theſe temperatures I exhibit in two ſeparate tables, the firſt indicating thoſe of the vernal equinox and of the northern tropic or midſummer, and the ſecond thoſe of the autumnal equinox and the ſouthern tropic or midwinter, over the Atlantic or ſtandard ocean in our hemiſphere.

Table

Table the First.

Vernal Equinox.			Midsummer.	
Latitude.	Mathem.	Real Mean.	Mathem.	Real Mean.
0	88°	84°	80,7°	83°
10	86,5	82	89,3	84,3
20	82,69	77	95,64	80,5
30	76,21	69,5	99,66	73,5
40	67,4	60	101,41	70,5
49	61	51	101,7	62 Lambert.
50	56,5	50,5	101,86	61
60	44	40	100,2	56
66°,33′	39,81	34	101,21	55 Lambert.
70	30,99	32	101,41	54
80	15,28	27	108,56	51
90	9,6 Lambert.	. . .	109,93	. . .

IN

In this table we fee, 1o. That during the vernal equinox the heat differs but little from the *mean* heat really obferved in all latitudes, and perhaps ftill lefs from the *maximum* of real heat. Yet, except in latitude 80°, it is always higher, both from the quantity of rays loft in paffing through the air, and from the quantity reflected by water and the frequent interpofition of clouds, &c.

2dly. We fee that the aftronomical heat conftantly increafes with the height of the latitudes, as the duration of the folar rays more than compenfates for their obliquity, when the fun is in the northern tropic; but the *real heat* decreafes as the latitudes increafe, becaufe this theoretic compenfation does not take place, from the interpofition of clouds and the accefs of cooler winds, and the increafed reflection from the furface of the water.

The different temperatures of different fummers are ultimately refolvable into the different direction of the winds during thofe feafons, and the different electrical ftates of the atmofphere, the fouth or fouth-eaft producing not only clouds which intercept the fun's rays, but alfo copious rains or hail, which, defcending from great heights and occafioning a copious evaporation, cool the air to a great degree. The north and north-eaft, on the contrary, unlefs immediately fucceeding great rains, (for then they increafe the

the evaporation) difperfe the clouds, and proceeding from countries then fomewhat heated, allow the fun's rays their natural calefactive effect. But why winds from oppofite points fhould prevail in different years, cannot be known, until the contemporaneous ftates of the atmofphere between the northern tropic and the equator, are known. It is poffible that frequent hurricanes and tornadoes, during which a quantity of air may be deftroyed and converted into water, may demand an annual fupply from the north, and thus occafion our north and eaft winds; and the abfence of thefe phænomena may occafion an influx from the fouth, if the north and eaft are fummoned to a different quarter, by fimilar caufes.

Table

Table the Second.

Autumnal Equinox.			Midwinter.	
Latitude.	Mathemat.	Real Mean.	Mathemat.	Real Mean.
0°	88°	84°	80,97	83°
10	86,5	84,6	69,66	78,5
20	82,69	80	58	72,5
30	76,21	73,5	44,54	64,5
40	67,41	70,5	30,55	54
49	61,	59	18,23	45 Lambert.
50	56,56	58,5	16,71	44
60	44	48	4,73	34
66,33	39,81	42	1,32	30 Lambert.
70	30,99	39	0, . .	27
80	15,28	33,5	. . .	22
90	9,6	. . .	. . .	. . .

ON

On this table we may remark, 1o. That though the mathematical temperature of the autumnal equinox be exactly the same as that of the vernal, yet the real is much higher, of the hemisphere being cooled during the winter is slowly heated, and being heated during the summer is slowly cooled.

2do. That in consequence of this circumstance, the real temperature of the autumnal equinox approaches much nearer to the astronomical than does that of the vernal, until we arrive at latitude 70° and the higher latitudes.

3tio. In all latitudes above the equator a cold approaching to the astronomical is scarce ever felt at sea in winter; to what can this be attributed but to the equatorial effluence? For other causes, viz. evaporation, and the frequent intervention of clouds, or at least haze, intercept the sun's rays, and consequently should cool the air even below the astronomical ratio which supposes the incidence of all the rays; in latitudes above 20° the difference is enormous.

4to. At the distance of some hundred miles from the coasts of the Atlantic, in latitudes above 40°, the cold is much more moderate than the mathematical ratios indicate, in most years; owing to the above-mentioned cause, and to the reign of westerly and

foutherly winds, which convey their heat to a confiderable diftance before they are cooled.

But in latitudes between 55° and 36° a degree of cold far fuperior to the aftronomical has often been obferved, and particularly of late, and in countries not very diftant, or even bordering on the Atlantic. Thefe extraordinary feafons may be attributed partly to the abfence of the fuperior effluence or its refrigeration in communicating with air replete with vapours, and partly to the prevalence of eaft north-eaft winds which proceed from the interior and coldeft parts of our continent, and hence the cold of the year 1776, fo well defcribed by Van Swinden, feems rather to have followed the order of the longitudes than of the latitudes. Wargentin, ecretary to the Royal Academy of Stockholm, informs him that the cold obferved that winter in Stockholm was not at all extraordinary, and expreffes his furprife that it fhould have been fo rigorous in Germany, France and England. Mem. Paris, 1776, p. 129. Nay it appears that the north-eaft wind which raged fo furioufly in Holland and at Montmorenci, latitude 49°, on the 27th (See Van Swinden, p. 40) had not been at Peterfburgh on the 18th, nor any day after; for a perfect calm reigned on that day, and the high winds of the remainder of the month were from the north-weft. Act. Petropol. p. 382. It is therefore probable that this wind proceeded obliquely from the eaftern

eaſtern and ſouthern parts of Ruſſia, and may have been derived from latitude 55° and longitude 40°, and originated on the Atlantic, lat. 40°, and hence the utmoſt rigour of this cold was ſooner perceived in the ſouth of France, as Tholouſe, Marſeilles, &c. than in the more northern latitudes, as may be ſeen in Van-Swinden's general table, for it reached theſe towns on the 18th or 19th of January. In the more northern latitudes it was felt only on the 27th; it is true its date at St. Jean de Luz, latitude 43°, is January 28th in the general table, but this is a miſtake, as may be ſeen, p. 181, for January 19th is there ſaid to be the true date, and, p. 179, it is ſaid that the 18th or 19th of January are the days on which the greateſt cold was obſerved in all places ſouth of the Garonne; which fully confirms my former ſtatement that the wind which produced this cold originated in the ſouth-weſt, and thence was gradually propagated northwards and eaſtwards. All the minuter modifications of this cold, in places not very diſtant from each other, may be aſcribed either to recent falls of ſnow, the proximity to which muſt effect more or leſs the thermometers, the greater or leſſer abundance of vapours in the atmoſphere, and other circumſtances too tedious and minute for inſertion in this general view.

Snow falling from ſome height in the atmoſphere is generally for ſome time ſurrounded with an atmoſphere much colder than

 the

the air fome feet above it, as Mr. Wilfon obferved, though it did not occur to him that the cold was communicated to the air by the fnow, for he thought it highly remarkable that a thermometer hung 24 feet above the fnow was four degrees lefs cold than one fufpended 2½ feet above it. Phil. Tranf. 1780, p. 462. Yet Mr. Boyle has long fince noticed a fimilar fact, (1 Boyle Abridg. p. 629) as related to him by fome navigators, and Fofter exprefsly mentions, that being to the leeward of an icy mountain (probably many feet diftant) the thermometer funk four degrees, and rofe to its former height when he had paffed that mountain, (Obfervat. p. 73) but when there is not a recent fall of fnow, the air feveral feet above the furface of the earth is generally colder (when no great evaporation takes place) than that nearer to its furface. Thus during the intenfe cold of January 1776, there having been no fall of fnow fince the 24th, Van Swinden found the degree of cold on the morning of the 27th to be 8°,25, while Camper, in the fame ftreet, whofe thermometer was fome feet nearer to the earth, found it only 6°,5. See Van Swinden, fur le froid de L'Annee 1776, p. 24, 25, 28 and 176.

Section VII.

Of the Temperature of the Southern Hemiſphere.

That the ſouthern hemiſphere is colder than the northern is the general opinion. 1 Gentil Voy. p. 114. That it is ſo, during its ſummer ſeaſon, can ſcarce be denied, at leaſt in latitudes above 40°.

But the winter of this hemiſphere ſeems not to be as cold as ours between the ſame parallels (ſee Eſtimate of the temperature of different latitudes, p. 51) the higher latitudes are rarely navigated during their winter.

Œpinus, reaſoning on aſtronomical principles, attributes the inferior temperature of the ſouthern hemiſphere to the ſhorter abode of the ſun in the ſouthern tropic, ſhorter by ſeven days, which produces a difference of fourteen days in favour of the northern hemiſphere, during which more heat is accumulated; and hence he infers that the temperature of the northern hemiſphere is to that of the ſouthern as 189,5 to 175,5 or as 14 to 13. Mem. Peterſb. 1761.

On

On the other hand, Mr. *Prevoſt*, of Geneva, and of the Academy of Berlin, grounded on phyſical conſiderations, namely, that the ſouthern winds incroach three degrees on the northern ſide of the equator, and that the north trade wind reaches no farther than to the fifth degree, even on the north ſide of the equator, infers that the temperatures of the north and ſouth ſegments, taken on theſe limits, are to each other inverſely as the ſegments, and hence calculates the temperature of the north ſegment to be to that of the ſouthern as 11 to 9. 38 Roz. Jour. p. 369. He would have ſtated this ratio * ſtill higher, if he had obſerved, as *Moore* does in his late excellent Treatiſe on Navigation, that the northern trade wind extends only to the tenth degree

* Some, I know, object that no ratio of heat can be juſt, becauſe its firſt original degree cannot be found by obſervation; but of what quantity can the abſolute unit be rigorouſly found? Muſt we therefore diſcard the units of weights and meaſure? The higheſt degree of cold muſt vary in various ſubſtances like fuſibility, for it muſt be that, beyond which the attractive power of their particles ceaſes, the reſiſtence to a nearer approach being infinite; now the force of this power muſt and does vary in various bodies, and therefore the point or degree at which it reaches itt *maximum* muſt vary alſo; with reſpect to air, I have reaſon to think it does not exceed 335 degrees below the freezing point. With reſpect to other bodies, 72° beneath the freezing point may to moſt practical purpoſes be aſſumed as the *maximum* of cold, being the degree at which mercury freezes.

degree of the north latitude, more or lefs according to the fun's declination, p. 126.

VARIOUS obfervations tend to juftify both thefe ratios, when applied to different latitudes, but neither of them apply within certain other latitudes.

THE following table exhibits, as far as I can collect, the mean heat of the correfponding latitudes of each hemifphere at fea, during the fummer months of each, extracted as to the fouthern hemifphere, from the journals of Cooke, La Peyroufe, Dalrymple and Marchand; in low latitudes the winter months alfo.

SOUTHERN.

Latitude.	Degrees.	SOUTHERN.	NORTHERN. Correſponding Months.
		December.	*June.*
From Lat. to	0° 15°	From the 9th to the 15th, incluſively Mean - 83°,57 3d Cook's Voy. Table 6th.	I cannot exactly ſtate the mean of the firſt 10 degrees during this month, but from latitude 10° to 15° the mean is - 84°,
	14°	From the 7th to the 20th - 78°,5 La Peyrouſe and in Cook - 85°, Mean - 81°,7	Mean - - 83°,5 This agrees nearly with *Epinus's* determination.
		April.	*October.*
	18°	From the 9th to the 23d, incluſively Mean - 81°,18 3d Cook's Voy.	Mean of the Month - 80°,
		July.	*January.*
	22° 23 24 25 and 26	From the 18th to the 27th, incluſively, 4 days in lat. 22°, 2 days in lat. 24°, 2 days in lat. 25°, and 2 in lat. 26°. Mean heat - 72°,5 3d Cook's Voy.	Mean heat of an equal number of days in ſimilar latitudes, north 67°, Hence the ſouthern winter is warmer than the northern.

March

Latitude.	Degrees.	SOUTHERN.		NORTHERN. Corresponding Months.	
		March and April.		*September and October.*	
From Lat.	0°	Five laſt days of March and ſix firſt days of April, mean -	71°,	Mean heat of an equal number of days - - -	75°,
to	27°	La Peyrouſe's Voy.			
		March and April.		*September and October.*	
	34°	15th March - -	69°	Mean of both months equal -	69°
		3d April - -	76	Of September - -	72°,5
		10th do - -	69		
		Cook's 4th and 19th table, mean	71		
		But would certainly be leſs if the whole of April were comprehended.			
		1ſt of March (Marchand) -	72°,5		
		May. Cape of Good Hope.		*November.*	
		Per Sparman - -	58°	Mean heat - -	62°,
		June.		*December.*	
		Per Sparman, mildeſt winter	57°	Mean - - -	60°,

Latitude	Degrees.	SOUTHERN.		NORTHERN. Correſponding Months.	
		Auguſt.		*February.*	
From Lat.	0°	Mean (per Dalrymple) -	62°,7	Mean - -	63°,
to	34°				
		November, December, January and February.		*May, June, July and Auguſt.*	
		Per Stavorinus's account of the mean of the aggregate of the four months - -	74°,5	Mean of the aggregate at ſea	72°,5
				That of land is higher.	
		December the 3d, (Cook's)	64,		
		January 21ſt (La Peyrouſe)	72,5		
		March.		*September.*	
	39°	From the 5th to the 11th, per Cook - - -	67°75	Mean - -	71°,
		But the remainder of the mouth probably colder.			
		January.		*July.*	
	43°	From the 19th to the 31ſt, mean	59°,44	Mean of the ſame days -	65°,
		Hotteſt day -	67,5		
		Coldeſt - -	56		
		Cook.			

Latitude.	Degrees.	SOUTHERN.		NORTHERN. Corresponding Months.	
		December.		*June.*	
From Lat.	0°	From the 9th to the 22d (La Peyroufe), mean - -	50°,4	Mean - -	67°,
to	44°	And in lat. 42° and 43°, the hotteft day did not exceed -	55,6		
		December.		*June.*	
	48°	From the 15th to the 29th, mean	43°,	Mean - -	63°,
		January.		*July.*	
		From the 1ft to the 5th and from the 10th to the 15th, inclufive, mean - - -	47°,	Mean - - -	65°,
		La Peyroufe.			
		Janury and February.		*July and Auguft.*	
	58°	From the 29th Jan. to the 8th February, mean -	43°,7	Mean - - -	56°,5
		La Peyroufe.			

HENCE we fee that up to latitude 40° the temperature of the northern is to that of the fouthern nearly as 14 to 13,5, and from latitude 40° to latitude 50° as 11 to 9, and from latitude 50° to 60° as 8 to 6 nearly, in the fummer feafon.

C H A P. IV.

Of the Denſity of the Atmoſphere.

Denſity is the relation which the quantity of matter in any ſubſtance bears to the bulk of that ſubſtance. It might as well have been ſaid, that it is the relation that the bulk of any ſubſtance bears to the quantity of matter contained in it, had it not been that the quantity of matter remains unaltered while the bulk varies. As the *quantity* of matter in all ponderable ſubſtances is proportioned to the *weight* and is no otherwiſe diſcoverable, the weight is often ſubſtituted for the quantity of matter. Hence the *maſs* of any body is equal to its denſity multiplied into its bulk.

To compare with preciſion the bulk and quantity of matter in any ſubſtance, it is neceſſary that a given weight and a given bulk be aſſigned, in which the relation betwixt them is aſcertained and expreſſed in known weights and meaſures, which may ſerve as a *ſtandard* in all other caſes.

THE bulk of all elaſtic fluids is affected and varied both by a variation of *preſſion* and a variation of *temperature*; to aſſign therefore a general ſtandard for the denſity of theſe fluids, it is neceſſary to diſcover

difcover fome general circumftance in which a known preffure, a known temperature, and a known weight may be afcertained as nearly as phyfical refearches admit or demand. Now this known and general circumftance is the temperature of the congelation of water, or 32° on Fahr. thermometer, or o of Reaumur, and is invariable all over the globe. The known *preffion* is that of 30 inches Englifh of mercury, whofe fpecific gravity in temperature 60° or 62° is 13,6 nearly, on a bafis of one fquare inch Englifh meafure; a preffion which may be univerfally afcertained, as mercury may be eafily purified fo as to poffefs that fpecific gravity, at leaft very nearly, and minute precifion is in moft cafes unneceffary, at leaft in meteorology. And, laftly, the known *meafure* is 87 feet, or 1044 inches, or 14,5 Englifh fathom, this being the quantity of air which in this circumftance weighs as much as one-tenth of a cubic inch of mercury, that is, 344,32 grains at the level of the fea, at which the general mean height of the barometer is 30 inches.

THIS determination poffeffes this peculiar advantage, that if the preffion *varies*, the temperature 32° remaining unaltered, (as when the barometer ftands above or below 30 inches) the meafure or number of fathoms of air that poffefs this weight may be difcovered by the table of common logarithms, rejecting the characteriftic; for if the barometer ftands at any given height, then fubftracting from the four firft figures on the left of its logarithm, the four firft figures

figures of the log. of a number one-tenth lower, the remainder thus found gives fathoms, and the refidue, decimal parts of fathoms.

FATHOMS multiplied into 6 give feet, and multiplied into 72 give inches. The fathoms, feet, and inches are paralelipipeds, which, for the purpofe of finding their weight, I fuppofe to ftand on a bafis of one fquare inch; confequently the inches are cubic inches, of which 1728 make one *cubic foot*, as do 144 of thefe altitudinal paralelipipeds, feet; and fo do 24 of thefe paralelipiped fathoms. All this is commonly known, but is proper to mention here for greater clearnefs. That at the temperature of 32° the difference of the logarithms gives the height of air in Englifh meafure, has been fhewn by General Roy. Sir George Schuckburg indeed fuppofes this to happen at the temperature of 31°,24, but the difference is of no confequence.

THOUGH this fubject is perfectly elementary, yet fome examples and the folution of a few problems will be ufeful to thofe that are little accuftomed to it. In thefe I all along fuppofe the temperature conftant, namely, at 32°.

Examples.

Examples.

BAROMETER being at 30 inches, what is the length of a vertical column of air that weighs as much as the tenth of a cubic inch of mercury?

HERE 29,9 is the number which is $\frac{1}{10}$ lower than 30. Then log. 30—log. 29,9 gives the length required, in fathoms and parts of fathoms.

Log. 30 = 477121
Log. 29,9 = 475671

Then the length required is - 14,50 fathoms.

HERE 14, is the difference of the four firſt figures of the logarithm, and 50, that of the two laſt figures, and therefore decimal parts.

Note.—When the barometrical height or heights are marked by mixed numbers, their logarithms muſt be ſought as if they were integral numbers, and if one of them be an integer, it muſt be raiſed by the addition of a cypher or cyphers to the ſame number of places as the other has.

THUS in the above example 29,9 is conſidered as 299, and 30 as 300.

Second.

Second.

BAROMETER being 29,5, what is the length of a vertical column of air that weighs as much as $\frac{1}{10}$ of an inch of mercury ?

Here the number next lower is 29,4.

Then log. 295 = 469822
log. 294 = 468347

14,75 fathoms.

HERE the preſſure being diminiſhed the length is greater than in the former Example.

Third.

BAROMETER being 29,54, what is the length of a vertical column of air that weighs as much as $\frac{1}{10}$ of an inch of mercury ?

Here the number 29,54—0,1 = 29,44. Then log. 2954=470410
log. 2944=468938

Length = 14,72 faths.

Fourth.

Fourth.

BAROMETER 30,05, what is the length of a vertical column of air that weighs as much as $\frac{1}{10}$ of an inch of mercury?

Here 30,05—0,1=29,95. Then log. 3005=477844
Log. 2995=476397

Length = 14,47 fathoms.

Fifth.

BAROMETER 28,77, (which is its mean height at Geneva) what is the length of a vertical column of air that possesses the above-mentioned weight?

Here 28,77 — 0,1 = 28,67. Then log. 28,77 = 458940
Log. 2867 = 457428

Length = 15,12 fathom.

HENCE they breath, at a mean, lighter air than we do in Dublin, in the ratio of 1412 to 1450, at the temperature of 32°, and also drier.

Sixth.

Barometer 30, what is the length of a vertical column of air that weighs as much as $\frac{1}{20}$ of an inch of mercury = 172,16 grains?

—$\frac{1}{20}$=0,05 and 30 —,05=29,95. Then log. 3000=477121
Log. 2995=476397

Length = 7,24 fathom,

which is nearly the half of the weight of $\frac{1}{10}$ of an inch of mercury. In this manner, the length correfponding, to any other proportional part of an inch of mercury may be found. And reciprocally, the weight being given, the length of the vertical column poffeffing that weight may be found, firft feeing what proportional part of the weight of an inch of mercury, viz. 3443,2, the given weight makes.

Seventh.

Barometer 29,5, what is the length of a vertical column of air that weighs as much as one inch of mercury?

Here 29,5 — 1, = 28,5. Then log. 295 = 469822
Log. 285 = 454845

Length = 149,77 fathom.

Eighth.

Eighth.

BAROMETER 30, what is the weight of a cubic foot of air in this temperature (32°)?

WE have already ſeen in the firſt example, that 14,5 fathom of air weigh 344,32 grains, and alſo premiſed that one cubic foot = 24 fathom. Now as 14,5. fathom 344,32 grains :: 24. fathom 569,99 grains (nearly 570 grains). Barometer 29,5 a cubic foot of air weighs but 560,249 grains. See ſecond Example.

Note.—The weight of a cubic foot of air in this temperature being thus found barometer 30, the weight of a cubic foot of air at any other barometrical height *below* 30 *inches* (=B) may be found though not exactly, yet very nearly, the difference not exceeding half a grain, for as 30. B :: 569,99.x. Thus as 30.29,5 :: 569,99 to 560,49, which differs only by 0,241 from the truth.

AND if the barometrical height given *exceed* 30 inches, then as B.30 :: x. 569,99.

So alſo 144 altidudinal feet on a baſis of one ſquare inch, weigh, barometer 30, 569,99 grains; ſo alſo do 1728 cubic inches,

 from

from which the weight at any other barometrical height may in the ſame manner be deduced. Thus barometer 30 in temperature 32°, 1000 cubic inches of air weigh 329,855 grains; then if the barometer be at 29,5, then 1000 cubic inches of air will weigh by this rule 324,357 grains; for as 30. 29,5 :: 329,855. 324,357, but more exactly 324, 218; for ſince a cubic foot of air at barometer 29,5 weighs 560,249 grains; then as 1728. 560,249 :: 1000. 324,218; the difference however is inconſiderable.

To expedite calculations of the weight of air, I have formed the annexed table of the weight of 1000 cubic inches of air (equal 13,888 fathoms or 83,333 feet,) at the following barometrical heights.

Weight

Weight of 1000 *Cubic Inches of Air at Temperature* 32°.

Barometer.	Grains.	Barometer.	Grains.	Borometer.	Grains.
31,—	340,84	30,3	322,1	28,6	303,4
,9	339,74	,2	321,0	,5	302,3
,8	338,6	,1	319,9	,4	301,2
,7	337,5	29,—	318,8	,3	300,1
,6	336,4	,9	317,7	,2	299,0
,5	335,3	,8	316,6	,1	297,9
,4	334,2	,7	315,5	27,—	296,8
,3	333,1	,6	314,4	,9	295,7
,2	332,0	,5	313,3	,8	294,6
,1	330,9	,4	312,2	,7	293,5
30,—	329,85*	,3	311,1	,6	292,4
,9	328,7	,2	310,0	,5	291,3
,8	327,6	,1	308,9	,4	290,2
,7	326,5	28,—	307,86	,3	289,1
,6	325,4	,9	306,76	,2	288,0
,5	324,3	,8	305,66	,1	286,9
,4	323,2	,7	304,5	26,—	285,8

* More exactly 329,810.

Barometer.	Grains.	Barometer.	Grains.	Barometer.	Grains.
26,9	284,7	24,—	263,8	23,1	242,9
,8	283,6	,9	262,7	22,—	241,8
,7	282,5	,8	261,6	,9	240,7
,6	281,4	,7	260,5	,8	239,6
,5	280,3	,6	259,4	,7	238,5
,4	279,2	,5	258,3	,6	237,4
,3	278,1	,4	257,2	,5	236,3
,2	277,0	,3	256,1	,4	235,2
,1	275,9	,2	255,0	,3	234,1
25,—	274,8	,1	253,9	,2	233,0
,9	273,7	23,—	252,8	,1	231,9
,8	272,6	,9	251,7	21,—	230,8
,7	271,5	,8	250,6	,9	229,7
,6	270,4	,7	249,5	,8	228,6
,5	269,3	,6	248,4	,7	227,5
,4	268,2	,5	247,3	,6	226,4
,3	267,1	,4	246,2	,5	225,3
,2	266,0	,3	245,1	,4	224,2
,1	264,9	,2	244,0	,3	223,1

Barometer.	Grains.	Barometer.	Grains.	Barometer.	Grains.
21,2	222,0	20,4	213,2	19,6	205,4
,1	220,9	,3	212,1	,5	204,3
20,—	219,8	,2	211,0	,4	203,2
,9	218,7	,1	210,9	,3	202,1
,8	217,6	19,—	209,8	,2	201,0
,7	216,5	,9	208,7	,1	200,9
,6	215,4	,8	207,6	18,—	199,8
,5	214,3	,7	206,5		

AND in general the weight of 1000 inches, barometer 30 being given, the weight at any proportional part of 30 may be found, it being the ſame proportional part of the weight at 30.

THUS the weight at barometer 30 is 329,855 then 20 being $\frac{2}{3}$ of 30 the weight at barometer 20 is $\frac{2}{3}$ of 329,855 grains=219,8 grains as in the table. The weight at barometer 10 (being $\frac{1}{3}$ of 30) is 109,7 grains, the weight at barometer 1 is 10,99, the weight at barometer 0,1 is 1, 099 grains.

THE weight of 1000 inches ſtanding on a baſis of one ſquare inch, barometer 30 is 329,855 grains.

Note.

Note.—Alſo that 1000 inches = 83,3333 feet ſtanding on the ſame baſis, and conſequently poſſeſs the ſame weight, as do alſo 13,8888 fathom.

PROBLEM, I.

To find the height that mercury would ſtand at, in a barometer at a given elevation in the atmoſphere, the height at which it ſtands at the bottom of that elevation being alſo given, and the mean temperature of the air being alſo 32°.

SOLUTION.

IF the height be given in feet or inches, convert them into fathoms or parts of a fathom. Subtract theſe fathoms (or parts of a fathom) from the log. of the height of the lower barometer, the remainder will give a logarithm belonging to the natural number, denoting the inches at which mercury ſhould ſtand in a barometer on that elevation.

Example.

BAROMETER below being at 30 inches, at what height ſhould mercury ſtand, on an elevation of 1000 feet? Mean temperature 32°.

HERE

HERE 10000 feet = 1666,66 fathom. And log. 30 = 477121. And 4771,21 — 1666,66 = 3104.55.

Now in Sherwin's table I find log. 31045 correſponds with the natural number 20,439 at this height, therefore the mercury in the upper barometer will ſtand.

PROBLEM, II.

THE height of mercury in a barometer on a certain elevation being given, to find the height of the mercury in a barometer at the bottom of that elevation. Mean temperature, 32.

Solution.

CONVERT the feet or inches expreſſing the elevation into fathoms, or parts of a fathom. Add theſe fathoms, &c. to the four firſt figures of the log. of the number of inches at which the mercury ſtands in the upper barometer, the ſum will give the logarithm of the height of the mercury in the lower barometer, which ſeek in the table,

Example.

THE upper barometer ſtanding at 20,439 on an elevation of 10000 feet, at what height does it ſtand in a barometer at the bottom of that elevation. Mean temperature 32°?

 Here

Here 10000 feet = 1666,66 fathom. And the log. of 20,439 is 310459. Now 310459 + 166666 = 477125 which correspond with the natural number 30. This then is the mercurial height in the lower barometer.

Corollary.

HENCE an elevation, and the mean temperature of 32° being given, the height at which two barometers would ſtand may be found, if the height of either be alſo given; otherwiſe the problem is undeterminate, and recourſe muſt be had to the general poſition, that the mean height of a barometer ſtanding at the level of the ſea is 30 inches.

But note.—That if the height of the mercury in both barometers be not equal, they muſt be equalized as to that circumſtance, by extracting the exceſs of one over the other, ariſing from the expanſion of mercury by heat. The method I employ of effecting this equation is ſimilar to that deviſed by Dr. Horſely; namely, by ſubſtracting the degrees of heat of the colder barometer, from thoſe of the warmer, and multiplying the difference into 0,40, and ſubtracting this product from the difference of the logarithms.

PROBLEM

PROBLEM, III.

THE height at which mercury ftands in two barometers, one at the fummit of any elevation, and the other vertically under it, or at leaft not more than ten miles diftant from a vertical fituation (the nearer the better), and the mean temperature being 32°; to find the meafure of that elevation, and the weight of of the intermediate column of air.

SOLUTION.

FIND the logarithms of thofe mercurial heights. The difference of the four firft figures on the left of thofe logarithms gives the meafure of that elevation in Englifh fathoms; and the difference of the remaining three figures gives the decimal parts of thofe fathoms, the whole multiplied into fix gives feet, or multiplied into 72 gives inches, as already faid. The height of the mercury in both barometers fhould be taken at the fame inftant, after allowing them fufficient time to affume the temperature of the air, or at leaft to cool.

To find the weight of the intermediate column of air, obferve by how many inches, or parts of an inch, the mercury

in the upper barometer differs from its height in the lower barometer. The weight of the intermediate column of air is just equal to the weight of that number of inches, or parts of an inch of mercury. Now a cubic inch of mercury, temperature 32°, weighs 3443,2 grains; therefore if the mercury in the upper barometer has fallen *n* inches, or parts of an inch, the intermediate column will weigh 3443,2 *n*. Thus if the mercury has fallen only $\frac{1}{10}$ of an inch, the weight of the intermediate column of air is $3443,2 \times 0,1 = 344,32$ grains.

Problem, IV.

The elevation being given, and the height of the mercury above and below being found, to find whether the mean temperature of the intermediate column of air were above or below 32° or at 32° exactly.

Solution.

If the number of fathoms in the elevation *exactly corresponds* with the difference of the logarithm of the mercurial heights then the mean temperature of the column was exactly 32°. If the number of fathoms in the elevation *exceeds* the number indicated

indicated by the difference of the logarithms, then the mean temperature was higher than 32°. If it falls ſhort of the number indicated by the difference of the logarithms, then the mean temperature was below 32°.

SECTION, II.

HAVING found and determined by its weight the denſity of air and that of its variations, as far as theſe may be occaſioned by the natural variations of *preſſion* in the temperature of 32°, it now remains to determine how far under each degree of preſſion it is altered by variations of temperature above and below 32°.

THERE are few ſubjects on which the opinions of philoſophers differ ſo widely as on this. The principal reaſon of which variance appears to me to have been, that the experiments they appeal to were made in manomeeers of different dimenſions in the bore and of glaſs differently compoſed and differently heated during its fuſion. From this laſt circumſtance its conducting power with reſpect to heat is various; but in all ſorts of glaſs veſſels air adheres to them with more or leſs force, according to their compoſition, in low heats (that

(that is beneath 170°), and most in the lowest, and in those whose bore is smallest; hence much heat is lost in vanquishing this adherence, and, when it rises or falls, it does so by irregular jumps or starts, to say nothing of the internal inequalities of the bore, nor of the adherence of the mercury which confines the air.

THE person who, as appears to me, has most successfully avoided or lessened most of these obstacles is Mr. Schmidt, Professor at Giessen. The description of his apparatus is too prolix for insertion and cannot be understood without a plate. It may be seen in 4 Gren's Journal. His experimeuts were made partly on air artificially dried, and partly on air artificially moistened, but the degree of moisture was determined by no known hygrometer. However he found the expansions of dry air to be exactly proportional to the degrees of heat it was exposed to, and in this point he agrees with D'Amontons, Lambert and De Luc. Sir George Schuckburg, abating some inconsiderable variations, draws the same conclusion in all heats from 32° to 83° under a pression of 30,5 inches of mercury, and infers from the mean of thirteen experiments that 1000 measures of air gain 2,43 by each degree of Fahr. above 32° and gain the same increase under a pressure of 40 inches or 23,5 inches of mercury. This last point however is contradicted

dicted, and as I think, very reaſonably, by D'Amontons, Lambert, and lately in an excellent memoire of Monge in the 5th vol. of the *Annales de Chemie*.

By the experiments of general Roy, and thoſe of Prieur Duvernois, 1 *Ann. Chemie*, the expanſions of air are various at various degrees of heat. See Philoſ. Tranſactions, 1777. The general alſo aſſerts, that air under a preſſure of 30 inches of mercury, or only of 25 inches, undergoes ſimilar expanſions, p. 708.

Rejecting manometers, as I think they ſhould be rejected, it appears to me that there are three teſts by which the ſuitableneſs of any coefficient ſhould be tried. The firſt is conformity with the weight found at 32°. The ſecond is the weight of the ſame volume of air at different temperatures, found by accurate weighing; and the third is the agreement of barometrical menſuration calculated according to the given coefficient, with the geometrical menſuration.

Note.— The difference betwixt 32° degrees and any degree above or below it I ſhall call in the ſequel the *fundamental difference*.

Examination

Examination of Schuckburg's Coefficient by the firſt Teſt.

Firſt Experiment.

Barometer 29,27 inches (29·3) } Thermometer 53° } 1°. He found a bottle containing 13558,5 grains of water, and conſequently containing 53,55 cubic inches. As 253,18 grains of water occupy the ſpace of a cubic inch, temperature 62°, to weigh 16,22 grains leſs when exhauſted of, than when full of air, after making every neceſſary allowance. Philoſ. Tranſ. 1777, 560. Conſequently 53,55 cubic inches of air of that temperature, and under that preſſion, weighed 16,22 grains. Now if 53,55 cubic inches weigh 10,22 grains, 1000 cubic inches ſhould weigh 302,89 grains, and ſhould retain that weight under any diminution of volume by cold, as there is no diminution of the quantity of matter.

2do. To reduce theſe 1000 inches of air to the volume they would occupy at 32°, I find that 53° exceeds 32° by 21 degrees; then ſubtracting from 1000 inches the number of inches, which according to Schuckburg they gained by twenty-one degrees above 32, namely $2{,}43 \times 21 = 51{,}03$ inches, we have the volume they would occupy at 32°, namely 948,97 (ſay 949) inches. And theſe ſhould weigh 302,89 grains.

3tio.

3tio. Now by the table, 1000 cubic inches at 32°, barometer 29,3 weigh 322,1 grains; and consequently 949 cubic inches ſhould weigh 305,67 grains. Therefore the error of Sir George Schuckburg's coefficient is exactly 305,67—302,89 = 2,78 grains in defect; therefore his coefficient retrenched too little, and ought to be higher. It is needleſs to examine General Roy's coefficient at this temperature, though it being higher (2,51) it muſt be leſs defective.

The coefficient which agrees beſt with this experiment is 2,84.

The eaſieſt method of finding how many meaſures, 1000 or any other meaſures of air gain by any temperature above 32°, is to multiply 1000, or ſuch other meaſures, into that fundamental difference, and divide the product by 400, that being the reciprocal of the coefficient $\frac{2,5}{1000}$, which is that I employ; as applying more exactly in general than any other, though in ſome caſes others may be more exact.

Second Experiment.

Barometer 28,77 } Thermom. 68° } 1°. According to Sauſſure, hygrometer. p. 284, a cubic foot of *dry* air weighs 751 grains, barometer 27, thermometer 16°.

Or in English measures, &c. 2090,8 cubic inches weigh 616 grains, barometer 28,77, thermometer 68°, consequently 1000 cubic inches should weigh 294,62 grains.

2do. The difference between 68° and 32° is 36°. Then by Schuckburg's coefficient $2{,}44 \times 36$, should be substracted from 1000 inches to reduce them to the volume they would occupy at 32°, namely 87,48 inches. Now $1000 - 87{,}48 = 912{,}52$ inches.

3tio. By the table, 1000 cubic inches at 32°, under the pression of 28,7 mercurial inches, weigh 315,5 grains, and consequently 912,52 inches should weigh 287,90, but the number found by Schuckburg's rule weighs 294,62. Its aberration therefore amounts to 6,72 grains in excess. This however may proceed from the circumstance that this air was perfectly *dry*, and therefore heavier than atmospheric air usually is.

The coefficient for this air, which is far less expansible than moist air, seems to be 1,29.

Third Experiment.

$\left.\begin{array}{l}\text{Barometer } 29{,}84\\ \text{Thermom. } 54{,}5\end{array}\right\}$ According to Lavoisier (See the French translation of my Essay on Phlogiston, p. 39.) 100 cubic inches of common air weigh 46 grains, barometer 28, thermometer

thermometer 10°, or in equivalent English denominations 121 cubic inches weigh 37,73 grains, and consequently 1000 cubic inches weigh 311,81. But this determination not being the result of single experiments made in these circumstances, but a mean collected from experiments made at different barometrical heights and in different temperatures, is not sufficiently rigorous to serve as a test in this case.

Fourth Experiment.

Barometer 30,32 } Thermom. 68°. } Hygrom. 75 } 1o. This is one of ninety-eight experiments made by me with an excellent air pump of Haafs's in the year 1786, on the weight of air at different barometrical heights from 30,36 to 29,03, and at different temperatures from 73° to 41°. The receiver contained 116 cubic inches very nearly, which weighed 34,75 grains; consequently 1000 cubic inches should weigh 299,56 grains.

2do. 68° surpass 32° by 36 degrees, consequently deducting from 1000 what they gained by 36° of heat (namely by Schuckburg, $2{,}43 \times 36 = 87{,}48$) there remain 912,52, which at temperature 32° weigh 299,56 grains.

 3tio.

3tio. Per table, 1000 cubic inches at 32°, barometer 30,3, weigh 333,1 grains, therefore 912,52 ſhould weigh 303,96 grains. The error of this coefficient is therefore 303,96 — 299,56 = 4,4 grains in defect; it retrenches therefore too little.

I SHALL quote no more of theſe experiments, as, not having made them with a view of ſo nice a teſt as the preſent, I cannot anſwer for their accuracy to half a grain, and the greateſt nicety would be requiſite. Yet I believe, upon the whole, this to be a better method of finding a coefficient than any manometrical experiments.

Method of finding the Coefficient.

1°. THE barometrical height being found to three decimal places, and the temperature from 80° to 33° true to one decimal place, and alſo the hygrometer, find the weight of 1000 cubic inches of air truly to $\frac{1}{10}$ of a grain = *W*.

2do. FIND the weight of 1000 inches at 32° at the ſame barometrical height = *t*, by the table, or ſtill more nicely. Now ſince *t* denotes 1000 inches, *W* alſo at 32° will denote a lower number of inches, ſince it is a lower weight; then ∺ *t*. 1000 :: W. N.

3tio.

3tio. Let the thermometrical degrees above 32° (that is their difference with 32°) $=$ D. and let $1000\ N = n$, then $D\, x = n$, and $x = \frac{n}{D}$. This gives the coefficient fought.

Example.

Thus in the firft experiment, barometer 29,3, thermometer 53°. $W = 302{,}89$ grains. and $t = 322{,}1$ grains. Now fince 322,1 grains denote 1000 cubic inches, 302,89 at the fame temperature fhould denote 940,36 inches $=$ N. and $1000 - 940{,}36 = 59{,}64$, the number of inches gained by 21° of heat; therefore putting x for the coefficient, $21\, x = 59{,}64$, and $x = 2{,}84$.

In the fecond experiment I find, by fimilar reafoning, the coefficient to be nearly 2,000, but as this relates to air much drier than any hitherto found in the atmofphere, it is only ufeful fo far as to fhew that the *moifter* air is the higher, muft be the coefficient. It varies therefore according to the ftate of the air as to moifture between 2. and 3.

In the fourth experiment, hygrometer 75°, I found it 2,77, but with us the hygrometer is generally above 80°. If, therefore, the hygrometrical mean be unknown, I take it that 2,5 is, at leaft of round numbers, the fafeft coefficient.

To

To prove the fitnefs of this coefficient it is neceffary to give fome inftances of barometrical menfuration, wherein it is juftified by refults nearly agreeing with geometrical menfuration.

Barometrical menfuration is founded on this principle, that the denfity of the atmofphere and the height of mercury in a barometer *decreafe* in a geometrical progreffion, correfponding with an *increafing* arithmetical progreffion as we afcend into the atmofphere; though it is only in the mean temperature of 32° that this geometrical progreffion can be truly indicated in Englifh meafures. The method therefore followed is, after equating both barometrical heights fo far as thefe heights are occafioned by difference of temperature*.

1°. To deduce from the corrected difference of the logarithms the heights refulting from the geometrical progreffion, as fhewn, p. 439. This is called the *Logarithmic refult.*

And 2dly, as this height is always inferior to the real height when the mean temperature of the atmofphere furpaffes 32°, to add to the logarithmic refult, the quantity which that column of air gains in confequence of its mean temperature being fome degrees

* The moft convenient mode of correction is fhewn, p. 438.

degrees above 32°, the quantity thus found is called the *calorific refult* or *fupplemental* height; the union of both gives the *compleat* height.

Hence we fee the neceffity of having four thermometers, one attached to each barometer to indicate the temperature of the mercury in each; and two detached, one at the top and the other at the bottom, to indicate the temperature of the air; the mean heat of both is fuppofed to indicate the mean temperature of the air, and the difference of this mean and 32° muft alfo be expreffed. This I call the *fundamental difference.*

3tio. The indications of the inftruments being minuted, and the barometers, or rather their logarithms, *corrected*, as fhewn, p. 438, the logarithmic refult is obtained by fubftracting the logarithm of the barometrical height at the top from that of the barometrical height at the bottom of the elevation, as fhewn, p. 439. This is alfo called the *approximate height.*

4to. To obtain the *calorific refult*, or *fupplemental* height, two products are neceffary. For, 1°. the fundamental difference muft be multiplied into 2,5, and divided by 1000; or, more fhortly, into ,0025, which is the fame thing. The reafon is this, 1000 meafures of air at 32° gain by a heat of one degree above 32°, 2,5 meafures;

meaſures; therefore by any higher number of degrees above 32° they gain ,0025 multiplied into that number. This is the firſt or *fundamental product.*

2do. THIS product multiplied into the logarithmic reſult, expreſſed in fathoms, gives the ſecond product, which affords the calorific reſult or ſupplemental height, and which I call the *ſupplemental product.* For ſince 1000 meaſures of air gain by multiplication into the number of degrees expreſſed in the *fundamental* difference, the quantity indicated by the firſt product, it is plain that the meaſures indicated by the logarithmic reſult would gain by multiplication into that product the quantity which reſults from that multiplication. This product therefore added to the logarithmic reſult in temperatures above 32°, or ſubſtracted if below 32°, gives the *compleat* meaſure of the elevation.

A FEW examples will render this proceſs more intelligible to many. I ſelect them chiefly from Sir George Schuckburg, becauſe he had the rare advantage of having the cotemporaneous indications of his inſtruments at one ſtation noted by intelligent perſons, while he himſelf obſerved them at another ſtation, namely, the higheſt. General Roy had ſometimes the ſame advantage, but I fear not always, and hence probably the defective meaſure of Mount Elio in Wales.

Meaſurement

Measurement of Mount Saleve.

First Series.

Lower Barometer 28,399. Log. 4535030
Upper - - - 25,712. Log. 4101359

Difference = 431,671
Correction — ,236

Logarithmic Result 431,435 fathoms.

Heat of the Mercury below 78°
- - - - - - - above 72°,1

5°,9 × ,4

Correction = 0,236

State of the Atmosphere and Calorific Result.

Heat of the Air above 65,0
- - - - - - below 73,9

Mean 2)138,9(69,4

Fundamental Difference 69°,4
— 32,
= 37,4

First Product.
37,4 × ,0025
= 0,0935

Second Product, or Logarithmic Result, multiplied into the first Product.
431,435 × 0,0935
= 40,334 fathom.

Compleat Height.
431,435
+ 40,334
= 471,769 fathom × 6 = 2830,614 feet.

Trigonometrical height = 2831,3 feet. Error of the barometrical measurement in *minus* 2831,3 — 2830,614 = 0,686 foot.

Third Series of Observations on Saleve. Philos. Trans. 1777, p. 531.

Lower Barometer 28,393 Log. 4532113
Upper - - - 25,690 Log. 4097641

Difference = 434,472
Correction — ,56

Logarithmic Result = 433,912 fathom.

Heat of the Mercury below 71°,1
- - - - - - - above 69°,7

Difference 1,4 × ,4

Correction = ,56

Calorific Refult.

Heat of the Air,		First Product.	Second Product, or Logarithmic Refult, multiplied into the firft Product.
above	62°	$35,2 \times ,0025$	$433,912 \times 0,088$
- - - - - below	72,5	$= 0,088$	$= 38,184$ fathom.
	2)134,5(67,2		
Mean	67,2		
	— 32,		
Fundamental Difference	= 35,2		

Compleat Height.	Error.
433,912	2832,576
+ 38,184	— 2831,3
= 472,096 fathom × 6 = 2832,576 feet.	= . . . 1,276 foot in *plus*.

Sum of the Errors of both Obfervations.

1,276 — ,686 = 0,590 of a foot.

These inftances evidently prove, that the errors of the barometrical calculation of heights proceed folely from the difficulty of obtaining the true mean temperature of the atmofphere, as there are hours in which the afcent of heat does not follow an arithmetical progreffion, as Pictet has fhewn, (fuppofing the inftruments truly read). The propereft hours are thofe between fun-rife and the hotteft hour of the day, or between that hour and fun-fet, the inftruments being fhaded from the fun, and left a fufficient time, (particularly the thermometers) to affume the temperature of the atmofphere.

Barometrical

Barometrical Measurement of the Height of the Mole. *The Geometrical Measurement gives its Height*, 4211,3 *Feet*, *per Schuckburg*. Philos. Transf. 1777, p. 580 and 581.

MOLE.

Lower Barometer 28,131 Log. 4491852
Upper - - - 24,178 Log. 3834204

Difference = 657,648
Correction — 1,84

Logarithmic Result = 655,808 fathom.

Heat of the Mercury below 61°,8
- - - - - - - - above 57,2

Difference 4,6 × ,4

Correction = 1,84

Calorific Result.

Heat of the Air below 63°,9
- - - - - - above 56,

2)119,9(59,9

Mean = 59,9
— 32,

Fundamental Difference = 27,9

First Product.
27,9 × ,0025
= 0,06975

Second Product.
0,06975 × 655,808
= 45,742

Error
4211,3
— 4209,3
= 2,0 feet.

Compleat Height.
655,808
+ 45,742
= 701,550 fathom × 6
= 4209,300 feet.

West Summit of Schihallion, from General Roy. Phil. Transf. 1777, p. 775. 11th of July.

Lower Barometer 29,595 Log. 4712183
Upper - - - 26,194 Log. 4182018

Difference = 530,165
— 5,400

Logarithmic Result = 524,765

Heat of the Mercury below 59°,5
- - - - - - - above 46,

Difference = 13,5 × ,4

Correction = 5,40

Calorific Result.

Heat of the air below 56,0
- - - - - - above 45,

2)101,(50,5

Mean = 50,5
— 32,

Fundamental Differ. = 18,5

First Product
18,5 × ,0025
= 0,04625

Second Product.
,04625 × 524,76
= 24,270

Compleat Height.
524,765
+ 24,270
= 549,035 fath × 6 = 3294,210 feet.
And by Trigonometrical Measurement = 3281,

Error of Barometer . . 13,210 feet.

Here the error of my method is 13,21 feet. Schuckburg's error is 11,5 feet, and Roy's—2 feet. But in calculating the height from the obfervations taken the next day, July the 12th, Roy's error was . . 29,5 feet, Schuckburg's . . 19,7 feet, and mine . . 20,6 feet. Phil. Tranf. 1778, p. 685.

Here it is evident, that thefe errors arofe from a faulty determination of the temperature of the atmofphere, for the approximate height found by General Roy, July 11th, was lower than that found July 12th. On the 11th it was 3142 feet, and on the 12th it was 3145 feet, and yet the compleat height was more defective. By my calculation it was higher the fecond day than on the firft, and yet the error was *minus*, though *plus* on the firft day.

I shall here add farther, that the error in calculating the height of Snowden, from the obfervations of Auguft 7th, two hours P. M. is by Schuckburg's method 8,1 feet, by mine 9,7, by Roy's 3,7; but by the mean of nine obfervations on different days, Roy's error is 6,1; the calculation that gave the beft refult was made on obfervations at nine o'clock A. M. and the worft were made on obfervations during a haze or fog, whether above or below; which fhews that the errors of thefe calculations proceed chiefly from the diforderly progreffion of heat into the atmofphere. Hence the winter feafon is not well fuited to them.

Having

Having given, p. 436, &c. the ſolution of ſeveral problems when the mean temperature of the atmoſphere is at 32°, I ſhall now ſhew how ſimilar reſults may be obtained when the mean temperature is above 32°.

Problem 1ſt.

To find the height mercury would ſtand at in a barometer on a given elevation in a known latitude in ſummer and in clear weather, when the height at which it ſtands at the bottom of that elevation and the temperature of the atmoſphere below are alſo given.

Solution 1°.

From the latitude deduce the height of the line of congelation, (ſee the table, p. 361,) and from the height of this line, and the temperature of the air below, deduce the temperature at the given height, as ſhewn p. 362. The temperature above muſt alſo be deemed that of the mercury in the upper barometer, as, if it were expoſed to the air a ſufficient time, it would certainly aſſume it.

2°. Convert the meaſure of the elevation into fathoms, and ſubſtract theſe fathoms from the logarithm of the height of

of the lower barometer, the remainder will give a logarithm which will denote the natural number, which expreſſes the height at which the mercury will ſtand in the upper barometer.

BUT, 3tio. The height of the mercury thus obtained is only that which it would have in temperature 32°. To obtain its height in the actual temperature above, we muſt add to the mercurial height thus found its $\frac{1}{9600}$* part, multiplied into the fundamental difference, viz. the difference of the temperature above with 32°.

Note The ſupplemental height will generally be obtained more eaſily by multiplying the fundamental difference into the logarithmic reſult, and dividing the product by 400. Or if Schuckburg's coefficient be preferred, by 411.

* This coefficient I take from that great treaſure of mathematical knowledge Dr. Hutton's Dictionary, article *Barometer*.

Examples.

Examples.

Saleve.

THIS mountain ſtands in the vicinity of Geneva and nearly in latitude 46°; its height over a certain point of the plain below was accurately meaſured geometrically by Sir George Schuckburg, and found to be 2831,3 feet. He alſo gave three ſeries of barometrical obſervations to aſcertain its height barometrically, from the ſecond of which ſeries the indications here given are taken. See Philoſ. Tranſ. 1777, p. 530.

HEIGHT of the lower barometer 28,390 inches.
Temperature of the atmoſphere below 73°.

THESE are the neceſſary *data*. The temperature above is part of the object of inveſtigation.

Inveſtigation of the Height of Mercury in the upper Barometer.

1°. THE temperature of the atmoſphere above, by the method given p. 362, was found to be 64,16 and by obſervation 64°, therefore I neglect the fraction as inconſiderable. This muſt alſo here be deemed to be the temperature of the mercury in the upper barometer, though by obſervation it was found higher, for

for that was owing to accidental caufes, viz. the heat of the tent in which it ftood, and that of the man that carried it, and not having been allowed time to cool.

2do. 2831,3 feet converted into fathoms = 471,833 fathoms.

Log. of 28,390 = 4531,654

Subftract the fathoms —471,833

Remainder = 4059,821

which is the logarithm of the height of the mercury in the upper barometer, and denotes the natural number and height of the mercury = 25,467 inches nearly.

But this height is that which the mercury would have in the temperature of 32°, and the temperature being 64° this height muft be increafed by $\frac{1}{9600}$ part multiplied into the fundamental difference, which in this cafe is 32°. Now $\frac{25,467}{9600}$ = ,00265, which multiplied into 32° gives ,0901, and this added to the mercurial height 25,467 gives 25,557, its true height at temperature 64°.

In this manner the barometrical heights at the altitudes of the line of congelation may be found. Thus the altitude of the line of congelation, latitude 0 being 28000 = 4666,66 fathom, if

if the barometer below be *at* 30 *inches* we have log. 30 = 477121 —4666,66 = log. 010455, which denotes the natural number 10,244; and if the lower barometer be at 29,8 we have log. 474216—4666,66 = log. ,007550, which denotes the natural number 10,176.

AND thus we find that the variations of the barometer at great heights are generally ſmaller than thoſe of the inferior barometer,* as De Luc and Sauſſure have alſo remarked.

THUS the variation of the inferior barometer in this caſe is 0,2 of an inch, and the correſpondent variation of the higher is only ,0068 of an inch.

YET ſometimes from local cauſes which diſturb the natural progreſſion of heat, the variations of the barometrical height above are greater or ſmaller, than, proportionally to the barometrical height below, they ought to be; but the difference is inconſiderable, though important, when the meaſurement of heights is aimed at.

PROBLEM II.

THE height of mercury in a barometer at any elevation in the atmoſphere being given or found, and alſo the height of mercury in a barometer vertically below that height, to find the

 weight

* 7 Sauſſure, 8vo. § 2049, p. 388

weight of the intermediate column of air, on a basis of one square inch, let the temperature be what it may.

SOLUTION.

THIS is found as in p. 439, and thus the weight of a column of air of any elevation and temperature, and under any given pression, may be known, first determining the height or number of inches at which mercury would stand on that elevation by the last problem.

THUS the weight of the intermediate column of air at Saleve, the barometer below being at 28,39 and above at 25,486, was 28.390—25,486 = 2,904 inches of mercury = 3443,2 grains × 2,904 = 9999,0528 grains or 20,810 troy ounces; whereas the weight at the mean temperature of 32° would have been 28,390—25,467 = 2,923 inches of mercury = 20,967 troy ounces.

Note.—The weight of a *vertical* column of air of a given length cannot be determined, unless the weight to which it is subject be known, and consequently the height at which mercury would stand in a barometer placed over it be also known; now this weight is variable according to the state of the barometer below, though the temperature should be constant.

THUS

Thus ſuppoſing the length of the vertical column 1000 feet, the weight of theſe 1000 feet will be greater when the barometer below ſtands at 30 inches, than when it ſtood at 29 inches, and conſequently the height of the barometer to which they are ſubject will be proportionably diminiſhed —Thus in temperature 32°, barometer 30, a vertical column of air of 1000 feet in length, that is 83,3333 feet or 13,888 fathom, weighs 328,8 grains, for logarithm 30=4771,213. 13,888 fathoms gives the log. 4757325, which correſponds with the natural number 29,9045 inches. And 30,0000 inches—$\overset{\text{Inches.}}{29,9045}$ = $\overset{\text{Inches.}}{,0955}$ and ,0955 of an inch of mercury = 3443,2 grains × ,0955 = 328,8 grains.—But when the barometer below is at 29, a vertical column of 1000 inches will be found by a ſimilar proceſs to weigh 317,9 grains, the upper barometer being 28,90772.

Table of the Heights of Air in Feet, which correspond with the Weight of one tenth of an Inch of Mercury or 344,32 Grains. from Barometer 31 to 15. Temperature 32°.*

Barometer.	Feet of Air.	Barometer.	Feet of Air.	Barometer.	Feet of Air.
31,—	—	29,4	88,5	27,8	93,6
30,9	84,2	,3	88,7	,7	93,9
,8	84,5	,2	89,1	,6	94,3
,7	84,7	,1	89,4	,5	94,5
,6	85,0	29,—	89,7	,4	95,0
,5	85,3	28,9	90,0	,3	95,2
,4	85,6	,8	90,3	,2	95,7
,3	85,8	,7	90,7	,1	95,9
,2	86,2	,6	90,9	27,—	96,4
,1	86,4	,5	91,3	26,9	96,7
30,—	86,7	,4	91,6	,8	97,0
29,9	87,0	,3	91,9	,7	97,4
,8	87,3	,2	92,2	,6	97,8
,7	87,6	,1	92,6	,5	98,1
,6	87,9	28,—	92,9	,4	98,6
,5	88,2	27,9	93,2	,3	98,8

Barometer.

*Taken from Sir George Shuckburgh's table. Phil. Tranf. 1777, p. 586.

Barometer.	Feet of Air.	Barometer.	Feet of Air.	Barometer.	Feet of Air.
26,2	99,3	24,3	107,0	22,4	116,0
,1	99,7	,2	107,5	,3	116,6
26,—	99,8	,1	107,9	,2	117,1
25,9	100,6	24,—	108,3	,1	117,7
,8	100,8	23,9	108,8	22,—	118,1
,7	101,2	,8	109,3	21,9	118,8
,6	101,6	,7	109,7	,8	119,2
,5	102,0	,6	110,2	,7	119,8
,4	102,4	,5	110,6	,6	120,4
,3	102,8	,4	111,1	,5	120,9
,2	103,2	,3	111,6	,4	121,5
,1	103,6	,2	112,1	,3	122,0
25,—	104,0	,1	112,6	,2	122,6
24,9	104,4	23,—	113,0	,1	123,3
,8	104,9	22,9	113,6	21,—	123,7
,7	105,3	,8	114,0	20,9	124,4
,6	105,7	,7	114,5	,8	125,0
,5	106,1	,6	115,1	,7	125,6
,4	106,6	,5	115,6	,6	126,2

Barometer.

Barometer.	Feet of Air.	Barometer.	Feet of Air.	Barometer.	Feet of Air.
20,5	126,8	18,6	139,7	16,7	155,6
,4	127,4	,5	140,5	,6	156,5
,3	128,0	,4	141,2	,5	157,5
,2	128,7	,3	142,0	,4	158,4
,1	129,3	,2	142,8	,3	159,3
20,—	130,0	,1	143,6	,2	160,4
19,9	130,6	18,—	144,3	,1	161,3
,8	131,3	17,9	145,2	16,—	162,4
,7	131,9	,8	146,0	15,9	163,4
,6	132,6	,7	146,8	,8	164,4
,5	133,3	,6	147,6	,7	165,4
,4	134,2	,5	148,5	,6	166,5
,3	134,6	,4	149,3	,5	167,6
,2	135,3	,3	150,2	,4	168,6
,1	136,1	,2	151,1	,3	169,8
19,—	136,8	,1	151,9	,2	170,9
18,9	137,5	17,—	152,9	,1	172,0
,8	138,2	16,9	153,7	15,—	173,1
,7	139,0	,8	154,6		

Table

Table of the Heights of Air in Feet, reaching from Barometer 32 Inches to Elevations where it would stand at one Inch, Temperature 32°. or the incorrect approximate Heights in other Temperatures.

Barometer. Inches.	Feet.	Barometer. Inches.	Barometer. Inches.	Barometer. Inches.	Feet.
32,—	00,0	31,—4	1336,6	29,8	2745,4
31,9	81,6	,3	1422,4	,7	2836,1
,8	163,4	,2	1508,6	,6	2927,0
,7	245,4	,1	1595,0	,5	3018,3
,6	327,8	30,—	1681,7	,4	3109,9
,5	410,4	,9	1768,7	,3	3201,8
,4	493,2	,8	1856,0	,2	3294,0
,3	576,3	,7	1943,6	,1	3386,6
,2	659,7	,6	2031,5	28,—	3479,5
,1	743,4	,5	2119,7	,9	3572,7
31,—	827,3	,4	2208,2	,8	3666,3
,9	911,5	,3	2296,9	,7	3760,2
,8	996,0	,2	2386,0	,6	3854,5
,7	1080,7	,1	2475,4	,5	3949,0
,6	1165,7	29,—	2565,1	,4	4044,0
,5,	1251,0	,9	2655,1	,3	4139,2

Barometer.

Barometer. Inches.	Feet.	Barometer. Inches.	Feet.	Barometer. Inches.	Feet.
28,2	4234,9	25,—	6432,6	23,8	8832,9
,1	4330,8	,9	6537,0	,7	8947,4
27,—	4427,2	,8	6641,9	,6	9062,5
,9	4523,9	,7	6747,2	,5	9178,1
,8	4620,9	,6	6852,9	,4	9294,1
,7	4718,3	,5	6959,0	,3	9410,7
,6	4816,1	,4	7065,6	,2	9527,8
,5	4914,2	,3	7172,6	,1	9645,5
,4	5012,8	,2	7280,1	22,—	9763,6
,3	5111,6	,1	7388,0	,9	9882,4
,2	5210,9	24,—	7496,3	,8	10001,6
,1	5310,6	,9	7605,1	,7	10121,4
26,—	5410,4	,8	7714,2	,6	10241,8
,9	5511,0	,7	7824,1	,5	10362,7
,8	5611,8	,6	7934,3	,4	10484,2
,7	5713,0	,5	8044,9	,3	10606,2
,6	5814,6	,4	8156,0	,2	10728,8
,5	5916,6	,3	8267,6	,1	10852,1
,4	6019,0	,2	8379,7	21,—	10975,8
,3	6121,8	,1	8492,3	,9	11100,2
,2	6225,0	23,—	8605,3	,8	11225,2
,1	6328,6	,9	8718,9	,7	11350,0

Barometer. Inches.	Feet.	Barometer. Inches.	Feet.	Barometer. Inches.	Feet.
20,6	11477,0	19,6	14138,2	17,6	17102,5
,5	11603,8	,5	14278,7	,5	17260,0
,4	11731,2	,4	14419,9	,4	17418,4
,3	11859,2	,3	14561,9	,3	17577,7
,2	11987,9	,2	14704,7	,2	17738,1
,1	12117,2	,1	14848,3	,1	17899,4
20,—	12247,2	18,—	14992,6	16,—	18061,8
,9	12337,0	,9	15137,8	,9	18225,2
,8	12509,1	,8	15283,8	,8	18339,6
,7	12641,0	,7	15430,6	,7	18558,0
,6	12773,6	,6	15578,2	,6	18721,5
,5	12906,9	,5	15726,7	,5	18889,1
,4	13041,1	,4	15876,0	,4	19057,7
,3	13175,6	,3	16026,2	,3	19227,5
,2	13310,9	,2	16177,3	,2	19398,4
,1	13447,0	,1	16329,2	,1	19570,4
19,—	13583,8	17,—	16482,1	Inches 15,—	19743,5
,9	13721,3	,9	16635,8	14	21541,3
,8	13859,5	,8	16790,4	13	23472,4
,7	13998,5	,7	16946,0	12	25558,1

Barometer. Inches.	Feet.	Barometer. Inches.	Table of different Feet.	Barometer. Inches.	Table of different Feet.
11	27825,4	30	000,0	19	11902,1
10	30309,0	29	883,4	18	13311,0
9	33054,4	28	1797,8	17	14800,4
8	36123,6	27	2745,5	16	16380,1
7	39603,1	26	3728,7	15	18061,8
6	43619,9	25	4751,0	14	19859,6
5	48370,8	24	5814,6	13	21790,7
4	54185,4	23	6923,6	12	23876,4
3	61681,8	22	8082,0	11	26143,7
2	72247,2	21	9294,2	10	28627,3
1	90309,0	20	10565,5		

Thefe two laft columns give the length of a column of air at 32°, counting from barometer 30 to a height in which the barometer would ftaud at (fuppofe) 29, = 883,4 feet.

Ufes

Uses of this Table.

10. To find the approximate height of any elevation in temperature 32°, the heights of the barometers above and below being given.

SUBSTRACT the number of feet oppoſite to the lower barometrical height, from thoſe oppoſite to the upper, the remainder is the approximate height very nearly. For as only one decimal place is given, there is no room for a correction.

Example.

THUS at the bottom of Mount Saleve a barometer ſtood at 28,39 inches (ſay 28,4) and at the top at 25,7 inches. Now in the table we have oppoſite to 25,7 inches 5713,0 feet, and oppoſite to 28,4 we have 3109,9 feet. Now 5713,0 — 3109,9 = 2603,1 feet, and Schuckburg makes it =2602,878 feet.

Supplemental Height.

MULTIPLY the fundamental difference of heat, into the approximate height, and divide the product by 450, if the height does not exceed 8000 feet; if it does, 1140 ſhould be the diviſor, the quotient is the ſupplemental height.

Thus in the above cafe the fundamental difference was 37,4, and the approximate height is 2603,1 feet. Now $\frac{37,4 \times 2603,1}{450} =$ 219,8, which added to 2603,1 = 2819 feet.

The error is 12 feet, and fcarce ever exceeds by 40 feet the refults of other methods.

2d°. To find the height or number of inches at which mercury would ftand on a given elevation true to five-tenths of an inch, when that altitude does not exceed 19700 feet, the height of the mercury below being given.—Add to the given altitude, that oppofite to the given barometrical height. The altitude neareft to the fum of both will point out the barometrical height fought.

Thus the height of Mount Blanc being 14624 feet above Mr. Senebier's cabinet at Geneva, and the height of the lower barometer being 29,1 (Englifh), a barometer at the top of that mountain was found to ftand at 17 inches—And by this calculation it ftood at 16,6, for oppofite to 29,1 we have 2475. And 14624+2475=17099. The neareft tabular number to this is 17102, which denotes 16,6 inches of mercury. The error then is 0,3 of an inch.—But in general it is much fmaller; thus, in meafuring the Mole, whofe height is 4211 feet, Schuckburg found the lower barometer 28 1, &c.

&c. and the upper 24,1. Now in the table the oppoſite number to 28,1 is 3386, which added to 4211 gives 7597, the neareſt tabular number is 7605, which denotes the mercurial height 23,9, the error then is only 0,2 of an inch.

3tio. THE barometrical height below being given, to find on what elevation the barometer above ſhould ſtand at a given number of inches, temperature 32°. This is in effect to ſeek the approximate height of that elevation, and therefore the elevation is found in the ſame manner. Thus the barometer below being at 28,4, the elevation at which it would ſtand at 25,7 is 2603 feet.

Note.—The table commencing at 30, I have inſerted becauſe we generally reckon upwards from the level of the ſea, at which the mean mercurial height, temperature 32°, is 30 inches. The intermediate tenths muſt be found by ſubſtraction from logarithm 30. Thus the height of mercury 29,1 denotes an elevation of 793 feet.

CHAP.

CHAP. V.

Of Precipitations from the Atmosphere.

THERE are five fubftances conftantly contained in the atmofphere, or at leaft in its lower ftrata, with which we are principally concerned; namely, oxygen, mephite, moifture, caloric, and electron, otherwife called the electric fluid, if this be not (which neverthelefs I fufpect it to be) a modification of caloric.* As, however, its effects are very different from thofe ufually afcribed to caloric, it may and ought, like ice and water, to be diftinguifh- by a different appellation. Befides thefe, heavy inflammable air and other miafmata are frequently found in it, and the lighter inflammabie air in its fuperior ftrata; but thefe mixtures are contingent, and therefore form no part of the prefent general inquiry.

Oxygen is frequently precipitated through its affinity to terreftrial fubftances in certain ftates, as combuftion, putrefaction, &c. Effects

* By *modification* I here mean a different fituation of caloric, namely, on the external furfaces of bodies, and not in their minuteft internal cavities. *Light* I take to be another modification of caloric, namely, an homogeneous arrangement of fome or all its conftituent particles, which, when thus arranged, and then only, become *vifible*.

fects foreign to meteorology. In the atmofphere, meeting with inflammable air, as in thunder ftorms, it is often converted into water by the electric explofion, and thus precipitated; and thus moft *fiery* meteors are formed.

The *electric fluid* is generally precipitated either fuddenly during electric explofions, or gradually and filently by the diminution of caloric and the fall of *dew* or *rain*. It is thefe laft precipitations which I now mean to examine; and as I afcribe them to electric agency, it will be neceffary to lay down thofe principles of atmofpheric electricity which are moft generally adopted by electricians, and the confequences that appear to me clearly deducible from them.

Section I.

Of Atmofpheric Electricity.

1°. Mere atmofpheric air is an *idio-electric*, or non-conducting fubftance.

2°. Air holding water in folution is alfo a non-conductor, and fo much the *more perfectly fo*, as, relatively to the degree of heat it poffeffes, it is farther removed from the point of faturation, and

and ſo much the leſs perfect as it is nearer to ſaturation with moiſture.

3°. As heat increaſes, and cold diminiſhes the ſolvent power of air, it follows that the quantity of moiſture in both caſes remaining the ſame, its non-conducting power is in direct proportion to its heat.

4°. Mere vapour is a *conductor* electricity, and ſo much the more perfect as it is *hotter*; and moreover, it contains and appropriates a conſiderable proportion of the electric fluid.

5°. Vapour diſſolved in air forms, as already ſaid, a compound which poſſeſſes the non-conducting property in the direct ratio of its heat and the inverſe of its proximity to ſaturation; this compound involves alſo a conſiderable portion of the electric fluid proportioned to the heat of the vapour in its naſcent ſtate.

6°. Vapours ſeparated from air, through whatever cauſe, form *clouds* and *foggs*, as already mentioned. In the firſt ſtage of their coaleſcence, the minute particles of which theſe conſiſt, retain, in the form of electric atmoſpheres ſurrounding each particle, a conſiderable proportion of the electric mattar which they previouſly, involved in their combined ſtate, and ſo much the greater, as they contained

contained more of it in that ſtate; but in this ſtate of coaleſcence they *retain* the electric fluid more ſlightly and become *conductors*, inſulated by the drier air that ſurrounds them.

7°. As the various ſubſtances which form the ſurface of the globe are variouſly heated, ſo alſo are the vapours they emit, and which are united to the air incumbent over them, conſequently they involve, when united to air, different portions of the electric fluid.

Conſequently 1°. The vapours emitted in different latitudes contain different portions of electric matter, bearing a porportion to the temperatures of thoſe latitudes.

2°. So alſo do the vapours emitted at different ſeaſons of the year.

3°. So alſo thoſe emitted from the ſea, and thoſe emitted from land between the tropics and the warmer tracts, if not mountainous; the former contain leaſt, and the latter moſt of the electric fluid.

4°. In the colder regions, in ſummer, the marine vapours alſo contain leaſt, and thoſe from land contain moſt of this fluid. But in winter the marine contain moſt, and thoſe from land leaſt.

5°. For the fame reafon the vapours that originate from high mountains contain leaft, and thofe from plains moft, and thofe alfo may vary in different fituations.

8°. Hence we fee that *clouds* originating from the condenfation of thofe vapours, and each retaining, though more loofely, at leaft a portion of the electric fluid they originally involved, muft contain different portions of it, or be, what is called, differently electrified, that is, *plus* or *minus*, at leaft relatively to each other.

9°. Clouds of equal furfaces and fimilarly and equally electrified repel each other. But if the furfaces be very unequal, the greater will gradually fteal the electron from the fmaller if brought into contact, as *Coulomb* has fhewn with refpect to metallic balls fimilarly electrified. Mem. Paris, 1787. If near, but not in contact, the greater will throw the leffer into a contrary ftate and then attract it.

10 . Clouds diffimilarly electrified, and beyond the ftriking diftance, attract each other.

11°. So alfo portions of air impregnated, but often far from being faturated with vapours unequally electrified, act upon each other, thofe more ftrongly electrified robbing the weaker of a portion

portion of their electron, and thus these are, at least in part, converted into a *cloud*; but clouds being still better conductors of electricity, the cloud thus formed robs, in its turn, the portions of air more strongly electrified of their electron, and converts, at least partially, their vapours into clouds; thus the dimensions of the original cloud is increased until it covers a considerable part of the horizon. By this sudden nubification a considerable quantity of caloric is set loose, which rarefies the air, and thus occasions a storm or erruption of air from all quarters, in a word, a *hurricane*.

Section II.

Of Dew.

In hot weather, particularly if the heat is long continued, as in summer and autumn, vapours are dissolved and elevated to great heights, but in the evening, particularly after sun-set, when the air is cooled, a portion of this vapour, proportioned to the refrigeration, gradually descends, until at last the lower strata of air become saturate and gradually deposit it on the contiguous denser substances. The moisture thus deposited forms what is called *dew*. That there is likewise an ascending dew arising from the effluvia of vegetables which emit air accompanied with moisture, but deposit it on the inferior surfaces of more elevated

 substances,

ſubſtances, has been ſufficiently proved and explained by Muſchenbroeck and Du Fay. Mem. Paris 1756.

There is however a puzzling circumſtance mentioned by the above-named naturaliſts, and for which they have not accounted. It has been obſerved in ſeveral places to be depoſited very copiouſly on idioelectrics, as glaſs and all vitrifactions, but ſcarce at all on the beſt conductors, as metallic ſubſtances, which evidently ſhews that dew conveys electricity; but why is not this fluid carried off by conducting ſubſtances more eaſily than by non-conductors? Mr. Achard tells us, Mem. Berlin, 1780, p. 17, that conductors aſſume the electrical ſtate of the ambient air very eaſily, but glaſs very ſlowly; therefore the conductors and vapours being both in the ſame ſtate, whether poſitive or negative, repel each other, but glaſs being in a contrary ſtate repels them. This, however, does not appear to me perfectly ſatisfactory, as far as reſpects glaſs, for it ſhould ſeem that, after ſome hours, this alſo ſhould aſſume the ſame electrical ſtate as the air, and then repel the vapours. I ſhould rather ſuppoſe that glaſs collects moiſture by reaſon of its ſtrong attraction for moiſture, for it has been obſerved that, in diſtilling water in glaſs and metallic veſſels of the ſame dimenſions and in the ſame heat, much more water paſſes in a given time through the glaſs than through the metallic veſſels: nay when the helm of a glaſs ſtill was connected with two receivers,

ceivers, with one by a metallic and with the other by a glafs adopter, more paffed through the latter than through the former. It is probable that Du Fay, who made thefe experiments, would give this explanation if he had purfued this inquiry. Thus the breath is immediately evaporated from polifhed fteel, but is retained by polifhed glafs.

Muschenbroeck alfo obferves that this different power of collecting dew is in fome meafure local, for that it does not take place at Leyden though it does at Utrect, Hefs and Paris, which appears to me to be occafioned by the unequal electrization of the vapours; thofe from the vicinity of Paris and Utrect being collected from a drier foil are more highly electrified than thofe of Leyden, the foil of which is fo moift that they can have no cellars, whereas they can and have at Utrect. He alfo tells us, fogs, univerfally, equally moiften all bodies, which fhews they contain much lefs electron than defcending vapors.

Hence dew is nothing more than the condenfed vapours elevated during the day from the foil over which the air that depofits it is incumbent, and hence its noxious qualities when elevated from ftagnating marfhes, and the various impregnations it has been found to contain.

Section

Section III.

Of the Haze of the Year 1783.

This year is remarkable for several extraordinary phœnomena of which this haze was the most universal. It is but slightly mentioned by English meteorologists*, but frequently by those on the continent, by whom it has been variously accounted for. The facts that relate to it, or appear to me to do so, are the following:

1°. In the months of February and March of that year happened the great earthquakes of Calabria, so minutely and exactly traced and described by Sir William Hamilton. Phil. Transf. 1783, p. 170, &c.

2°. During this earthquake the incumbent atmosphere was incumbered and obscured by clouds to an uncommon degree. 24 Roz. Journ. p. 5.

3°. On the 17th and 18th of the subsequent month of June a dark reddish haze that obscured the light of the sun was observed in many parts of Europe, even in high northern latitudes as England and Sweden, Phil. Transf. 1784. p. 285, 418, and 24 Roz. p. 8 and 405.

4°.

* Phil. Transf. 1784.

4°. This haze was in moſt places introduced by a ſouth or ſouth eaſt wind. 24 Roz. p. 5 and 409. Mem. Dijon, 1783, p. 228.

5°. This ſoutherly wind did not produce rain, nor lower the barometer, but on the contrary raiſed it to an unuſual height, and it fell when the wind changed to the north. 24 Roz. p. 4 5 Neve Schwd. Abhandl. p. 9 and 13. 2 Mem. Dijon, 1783 p. 214. 1 Mem. Lauſanne, p. 116 and 280.

6o. This ſoutherly wind when the haze firſt appeared occaſioned an extraordinary degree of cold, for the month of June. 23 Roz. p. 203, 24. Roz. p. 4. Phil. Tranſ. 1784, 417 and 418.

7°. This haze at firſt ſtood at an extraordinary height, in ſo much that it was not diſſipated or leſſened by rains or winds, but ſeemed of a dry nature; but after ſome time it gradually ſunk lower, and was at leaſt partially diſſipated by rains and winds. Phil. Tranſ. 1784, p. 285. 24 Roz. p. 4, 5, 9, 10, 407 and 408. 11 Mem. Dijon, 229. 1 Mem. Lauſan. 119.

8°. It was followed at certain intervals by thunder and ſhort but copious falls of rain, and fiery meteors, one of which is well traced and deſcribed by Sir Charles Blagden. See Phil. Tranſ. 1784, p. 201, 285 and 286. 1 Mem. Lauſanne, 114. 23 Roz. 203. 24 Roz. 5, 10, and 409.

9°. It

9°. It is even ſaid to have depoſited in ſome places a viſcid liquid of a diſagreeable taſte, 24 Roz. 20. 1 Mem. Dijon, 1783, p. 229. Nay ſome ſay it was accompanied by an acrid or ſulphureous ſmell, 23 Roz. Journ. p. 203, and 24 Roz. p. 410.

Other circumſtances are mentioned by the authors quoted, but with ſome variation, ſuch as muſt be expected to ariſe from different ſituations as well as from the different ſtates of this miſt.

From an attentive conſideration of theſe obſervations I am diſpoſed to concur with the opinion of Sir William Hamilton, Phil. Tranſ. 1783, p. 194 and 199. That this haze was cauſed by the immenſe quantity of inflammable air extricated from the bowels of the earth during the earthquakes of Calabria, ſtrongly electrified, and impregnated with ſulphureous bituminous, earthy and metallic particles. The quantity was ſuch as to diffuſe itſelf after a few months over moſt parts of Europe. While theſe heterogenous particles were held in ſolution the tranſparency of the atmoſphere was not altered; it was otherwiſe when they began to precipitate.

Then 1°. The obſcurity and dark red colour of the haze may be attributed to the ſulphureous metallic, &c. particles, which abſorbed all but the leaſt refrangible ſolar rays, particularly at ſunriſe and ſun-ſet.

2do. The

2do. The direction of the wind seems merely a contingent circumstance, yet as this phœnomenon, or at least its cause, originated in the southern part of Europe, it possibly has been extended northwards by a southern wind.

3°. While the *impetus* of the eruption of this air lasted, it may well be supposed to reach the greatest heights like the still denser smoke of volcanos; but, as it consisted chiefly of heavy inflammable air, it may be inferred that it gradually descended and its heterogeneous contents became visible every where nearly at the same time, but its height for a long time secured it from the winds that prevail in the lower strata of the atmosphere; but when it had descended some way these winds and rain gradually dispersed and washed it away.

4°. The atmosphere, thus loaded with metallic and other particles thrown into it far beyond the limits suited to their specific gravity, must have its weight increased, and hence the extraordinary heights of mercury in barometers. While diffused over vast spaces, the additional weight accruing to the atmosphere from these particles may have been insensible or not easily distinguished, but became evident when they had somewhat descended and were concentrated, and hence the varying accounts we have had of the manner in which barometers were affected during the mist;

neither can it be ſuppoſed that the denſity of theſe foreign particles was ever the ſame.

5°. THE uncommon cold produced by this haze originated, partly from the obſtruction it preſented to the paſſage of the ſolar rays, and partly from the deſcent of its particles exceedingly cooled in the lofty regions of the atmoſphere into which they at firſt aſcended; this effect, however, muſt have varied with their number, denſity, and the duration of their precipitation.

6°. THE thunder, fiery meteors and the ſudden and violent rains, which accompanied the deſcent of this haze, may well be attributed to the high electrical ſtate of its particles which were inſulated in the more elevated regions, but was often communicated in the lower and moiſter ſtrata; on this occaſion the bituminous and ſulphureous particles muſt have acted a principal part.

A SIMILAR haze was obſerved in Perſia in the year 1721, after the great earthquake in Georgia which deſtroyed the city of Tauris. 5 Richard's Hiſt: des meteors, p. 164. The darkneſs which obſcured the ſun after the death of Cæſar may probably be aſſigned to a ſimilar cauſe, for Julius Obſequens * de Prodigiis, tell us there were earthquakes about that time.

THE

* He lived in the reign of the emperor Honorius, and collected the prodigies mentioned by Livy, &c.

The only objection to this theory, which may at first view appear plausible, is, that the quantity of smoke elevated to the greatest height during volcanic eruptions does not produce a haze of a similar nature and extent; yet on consideration it appears to me of no weight; for, 1°. The electricity of this smoke is destroyed during the eruption by the frequent flashes of lightning that take place in the body of the smoke during its ascent, and consume the hydrogen and bituminous particles; and, 2do. This smoke then consists chiefly of heated air, impregnated with incinerated carbonic particles, and not of metallic particles of which it is not a solvent; and as the colder ambient air presses towards the volcano and the heated smoke, its diffusion is prevented, and never, unless by a storm, are its particles carried farther than 20 or 30 miles. See Histoire de Vesuve par Padre della Torre, p. 127, &c.

Section, IV.

Of Rain.

Diminution of the temperature of air, replete with moisture, below the degree at which its saturation takes place, whether this refrigeration were caused by rarefaction, or by the intermixture with colder air, has been generally supposed the cause of rain; but this hypothesis in both its branches

has been ſatisfactorily refuted: the firſt by Sauſſure* and the ſecond by De Luc†. And in fact refrigeration will indeed ſeparate moiſture from air cooled below the temperature neceſſary to hold it in ſolution. But this ſeparation will terminate, if gradual, in the production of *dew*, as already ſeen, or, if ſudden, in the production of a *cloud*, as in the experiment of Tornea mentioned by Maupertuis, but cannot nor has in any inſtance produced *rain.*

Rain is the immediate reſult of the union of the particles which form clouds; and this union is the conſequence of the ſubtraction of the electric atmoſpheres which keep them at a diſtance from each other; and this ſubſtraction is itſelf the conſequence either of the ſuperior attraction of better conductors or of the attraction or repulſion of other clouds through the cauſes mentioned in the firſt ſection of this chapter. The conſequence of the *attraction* of clouds is their *incorporation*, and the reſult of their incorporation is the increaſed volume of their conſtituent particles, an increaſe proportioned to the attraction that produced it; the increaſed volumes, thus produced, form thoſe drops whoſe collection we call *rain.* The weight of theſe being ſuperior to the reſiſtance of air, they neceſſarily deſcend, and the cauſe of their different ſize is thus clearly diſcerned.

The

* Hygrom. § 224. † De Luc Idées de Meteorologie, p. 43, &c.

The repulſion of clouds ſimilarly electrified, and not greatly differing in magnitude, terminates in a bare increaſe of diſtance; but, if their magnitudes be much diſproportioned, it may terminate in *attraction*, or at leaſt in forcing the conſtituent particles into cloſer contact, and thus by increaſing their magnitude effect the ſame reſult.

When the attraction takes place between clouds differently and highly electrified, and within what electricians call the *ſtriking diſtance*, the electric fluid is ſet free, the coaleſcence of the nubilous particles is more rapid and complete, and hence the large drops that follow flaſhes of lightning, or even floods, where the quantities both of vapour and electron are conſiderable, as between the tropics.

Upon theſe principles moſt of the phonomena relative to rain appear to me eaſily explicable; of theſe the moſt remarkable are:

1°. That rains are more copious but leſs frequent in the ſouthern parts of our hemiſphere not much elevated over the ſea, than in the more northern latitudes. They are more copious when their productive cauſes occur, evidently becauſe the quantity of ſuſpended vapour is much greater in the hotter than in the

the colder regions; but they are leſs frequent, becauſe the variations of wind in different directions which introduce and intermix clouds differently electrified are leſs frequent; this might be proved by inſtancing the rainy ſeaſons between the tropics, were it not that this illuſtration would extend this paper to too great a length. Even in moderately elevated ſituations between the tropics, if inſulated and of ſmall extent, as the iſland of St. Helena, it ſeldom rains.

2do. THAT, in the temperate latitudes, rains are alſo more copious, though commonly leſs frequent in *ſummer* than in *winter*, for the reaſons already aſſigned. *Dry* ſummers are then the conſequence of uniform winds, from whatever quarter they may blow, as *wet* ſummers are of their variation, particularly if in oppoſite directions, and if they reach heights ſufficient to intermix the clouds that ſubſiſted during the reign of their antagoniſts.

3do. SOUTHERLY winds are moſt frequently accompanied with rain, in moſt parts of Europe at leaſt, and probably in moſt parts of our hemiſphere; but *northerly* and *eaſterly*, with clear, dry, and ſerene weather. Becauſe ſoutherly winds are not only warmer, proceeding from warmer climates, but alſo more highly electrified than the ſoil of the colder countries into which they flow. Hence the copious vapours they contain are quickly deprived of part of their electron, and thus converted into clouds; but the

the ſuperior ſtrata of the atmoſphere under which the ſouthern air is introduced, not being ſupported by air as denſe as that which ſubſiſted under them before their introduction, neceſſarily deſcend and mix with the inferior ſouthern air; by this intermixture they are warmed, and deprive the clouds already formed and in its vicinity of part of their electron, or perhaps in conformity to the eleventh principle, they are themſelves deprived of part of their electron by thoſe clouds, and the vapours they contain are thus converted into clouds; in either way clouds differently electrified muſt be formed. Hence proceeds their gradual attraction to each other which terminates in thoſe gentle ſhowers that uſually accompany this wind. Northerly and eaſterly winds on the contrary, proceeding from colder countries are leſs highly electrified than the ſoil of the countries they invade; and hence from the oppoſite reaſons to thoſe juſt mentioned they introduce ſerene weather and a diſpoſition adverſe to nubification.

THE reaſons hitherto adduced to explain the different effects of theſe different winds, evidently aroſe from an ignorance of the origin and progreſs of theſe winds. It was imagined that ſoutherly winds, flowing into colder countries were ſuddenly cooled by an intermixture with the colder air of thoſe countries, and that thus their vapours were condenſed into rain, yet, even ſo, this intermixture could only produce clouds and not rain, but

but in fact this intermixture cannot take place, except with the superior and unmoved strata of the atmosphere, and these alone could not produce numerous clouds, much less copious rains; for the air of the countries into which these southerly winds flow, must itself have flown northwards, before the more southern air could enter upon them, as shewn p. 397.

Moreover, southerly winds retain much warmth, and northerly winds are so much colder in the countries into which they are introduced, that their temperature cannot be supposed sufficiently altered to deposit much vapour in the one case, or dissolve much of that already condensed in the other; on the contrary the warm southerly wind should dissolve the clouds already formed, and the northerly, by their encreased cold, should produce many more.

Hence electrical agency must of necessity be recurred to, though I do not doubt but it may be more correctly applied by persons better versed in electrical knowledge than I can pretend to be. Currents of air flowing in different directions at different heights in the atmosphere, must undoubtedly be intimately connected with these effects, but with these we are at present too little acquainted.

4°. That a disposition to rain is generally connected with a diminution of the weight of the atmosphere, as is a disposition

to

to ſerenity with the increaſe of its weight. Becauſe under the diminiſhed weight of the atmoſphere, the eruption of vapours both from land and water is much more copious, a diſpoſition highly favourable to nubification, and the clouds already formed deſcend lower, are more concentrated, and hence more diſpoſed to react upon and attract each other and thus produce rain. The increaſed weight of the atmoſphere muſt produce oppoſite effects and induce a diſpoſition adverſe to the production of rain.

5°. THAT more rain falls on the ſurface of the earth than on ſmall elevations above it, as from 30 to ſome 100 feet: ſee Phil. Tranſ. 1769, p. 361; and of 1771, 297; and of 1777, p. 256. This effect ſeems to me to proceed from the greater ſtillneſs and tranquillity of the air near the ſurface of the earth than at greater elevations. To prove this, it is only neceſſaty to collect the rain that falls in moderate weather on both ſituations, with that which falls on both, in more ſtormy weather. If this explanation be juſt the difference between the quantities collected in both ſituations will be found greater in the latter than in the former caſe. This experiment I ſhall make, and communicate the reſult to the Academy.

6°. THAT the quantities of rain collected at the top of high mountains, and on plains about half a mile diſtant from thoſe mountains are nearly equal, but in ſummer there falls ſomewhat

more on the plains, and in winter ſomewhat leſs. Phil. Tranſ. 1771, p. 295. The greater quantity of rain collected in ſummer on plains appears to me to proceed from the cauſe juſt mentioned, the leſs diſturbed ſtate of the atmoſphere; but the quantity of rain gained through the influence of this cauſe is often, in great meaſure, compenſated by that ariſing from the condenſation of fogs formed on the ſummits of mountains, particularly at night, when neither fog nor rain exiſt on the plains. But in winter, theſe miſts being much more frequent and denſer on the ſummits of mountains, the quantity of moiſture which they depoſit is far more conſiderable.

7°. THAT it rains much more on the weſtern coaſts of moſt parts of Europe, particularly if mountainous, than in the interior parts of thoſe countries, or on the eaſtern coaſts of the Britainnic iſlands.

THE cauſe of theſe phenomena is very obvious. Weſterly winds are by far the moſt frequent in moſt parts of Europe; theſe flow from the Atlantic which bounds it, and generally convey marine clouds electrified differently from the ſoil or land over which they flow, as alſo from that of the higher clouds under which they reign. Hence proceeds their mutual attraction, and thence rain. This effect muſt take place principally on the weſtern coaſts; when they

they proceed further, this different electrical ſtate muſt either ceaſe or be diminiſhed. When the coaſts are mountainous, theſe mountains quickly abſorb the electric matter contained in the weſtern blaſts, and by colliſion, condenſe the vapours they contain, firſt into clouds, and finally into rain. Hence it often happens that weſterly winds, particularly in ſummer, produce no rain, either becauſe they introduce no clouds, or meet with none differently electrified.

8°. THAT in ſome countries it ſcarce ever rains.—This ariſes from local circumſtances, as is apparent in the following inſtances: 1°. It never rains on the plains of Peru from the gulf of Guyaquil, nearly under the equator, up to latitude 23° ſouth, nor is thunder ever heard there, though theſe plains border on the Pacific Ocean, but they receive a ſlight dew every night. Bouguer, fig. de la Terre XXIII. 2 Ulloa's Mem. p. 157. 2 Phil. Tranſ. Abr. 132. Plainly from the following reaſons: Theſe plains are entirely ſandy, and, conſequently, emit very little vapour, being ſoon parched by the heat that there prevails; conſequently, the intermixture of marine vapours can produce no effect. Again, the clouds in theſe tracts are elevated to a great height, and are attracted by the electrical agency of the Cordellierres that border on theſe plains, to their lofty ſummits, and there produce copious rains; hence alſo the ſandy and extenſive deſarts of Arabia and Africa are ſeldom re-

 freſhed

freſhed by rain. 2do. It ſcarce ever rains in Egypt, particularly in Upper Egypt. Now it is to be obſerved, that Egypt is ſo ſituated betwixt lofty mountains that no wind can enter it without paſſing over them, but the northerly winds, which iſſue from the Mediterranean; for a ſoutherly wind muſt paſs over the mountains of Abyſſinia; an eaſterly, over thoſe that intercede between the Red Sea and the Nile, and proceeding from the Deſarts of Arabia, can convey little or no vapour; and weſterly winds muſt paſs over the Deſarts of Africa and Mount Atlas. Now the northerly wind does not begin to blow until the month of June, when Egypt is ſo ſcorched as to emit ſcarce any vapour, and the few clouds it may convey are attracted by the mountains of Abyſſinia. Towards the middle of June the inundation of the Nile, it is true, commences, and then, as the northerly wind ſtill continues, perhaps rain might be expected, but little attention being paid to it then, we are not informed whether any falls or not, perhaps the clouds then alſo paſs to the mountains of Abyſſinia, whither this wind conveys them, and which they deluge with rain; all other winds depoſit their moiſture on the reverſe of the mountains they paſs over.

CHAP.

C H A P. VI.

Prognoſtics.

WHEN the barometer falls, and the hygrometer riſes, *rain* is announced.

WHEN the barometer riſes, aud the hygrometer falls, we may expect fair weather, if farther changes do not appear in theſe inſtruments, as ſometimes there ſuddenly do.

IF the barometer falls and the hygrometer alſo, *windy weather* will probably follow—particularly if the barometer falls much below its natural height, which in Dublin is from 29,9 to 29,98.

AGAIN, in the morning the hygrometer is generally higher than at noon, by reaſon of the difference of temperature; but if it ſtands *lower at noon* in a greater proportion than the difference of temperature demands, it prognoſticates *fair weather*.—On the contrary, if at noon it be higher than it ſtood in the morning, *rain* may be expected. Sauſſ. Hygr. p. 356.

To

To foresee the Rise or fall of the Barometer in Day-time.

Observe it at feven o'clock in the morning, and afterwards at nine and at ten. If it remains fteady, its next motion will probably be *downwards*. So alfo if it falls within that interval of time, the probability is, that it will *fink* ftill lower. But if it rifes within that interval, the chances of a greater rife or of a greater fall are equal.

Again, obferve the barometer at one in the afternoon, and again at three; if it remains unmoved, it is probable that it will *rife*, but if it has fallen, the chances of a farther rife or fall are equal.

APPENDIX.

APPENDIX.

Of the Manner of taking Obſervations on the Hygrometer and of correcting its Indications.

THE hygrometer ſhould habitually be kept looſe under a glaſs jar, to prevent it from being fouled by flies or duſt. Obſervations ſhould be made in the open air and in the ſhade at a diſtance from houſes or reflected heat. It requires but about two minutes to arrive at its proper height. But the obſervations thus taken muſt be corrected by the table that follows:

TABLE

TABLE III.

Of the Degrees of Cold *necessary to bring the Hydrometer from its actual State to the Degree which marks Saturation.*

Hygrometer.	Cold.	Hygrometer.	Cold.	Hygrometer.	Cold.
XL	34,695	LV	21,825	LXX	12,846
XLI	33,642	LVI	21,135	LXXI	12,333
XLII	32,625	LVII	20,461	LXXII	11,829
XLIII	31,641	LVIII	19,801	LXXIII	11,333
XLIV	30,689	LIX	19,156	LXXIV	10,846
XLV	29,766	LX	18,524	LXXV	10,370
XLVI	28,870	LXI	17,905	LXXVI	9,904
XLVII	28,001	LXII	17,299	LXXVII	9,449
XLVIII	27,156	LXIII	16,705	LXXVIII	9,005
XLIX	26,334	LXIV	16,122	LXXIX	8,572
L	25,534	LXV	15,551	LXXX	8,151
LI	24,755	LXVI	14,990	LXXXI	7,741
LII	23,995	LXVII	14,439	LXXXII	7,340
LIII	23,254	LXVIII	13,899	LXXXIII	6,947
LIV	22,531	LXIX	13,368	LXXXIV	6,561

Hygrometer.	Cold.	Hygrometer.	Cold.	Hygrometer.	Cold.
LXXXV	6,182	XC	4,379	XCV	2,698
LXXXVI	5,810	XCI	4,034	XCVI	2,373
LXXXVII	5,444	XCII	3,694	XCVII	1,961
LXXXVIII	5,084	XCIII	3,358	XCVIII	1,399
LXXXIX	4,729	XCIV	3,026	XCIX	0,755

Notes—1°. THE degrees of heat here mentioned are thoſe of Reaumur.

2do. THAT the point of ſaturation is here ſuppoſed to be C.

3tio. THAT the ſecond column expreſſes the diſtance from ſaturation, that is, the number of degrees by which the heat muſt be diminiſhed to raiſe the hygrometer to C. As the degrees here mentioned are thoſe of Reaumur, theſe and thoſe of Fahrenheit muſt be converted into each other as occaſion may require*.

* To convert Fahrenheit's degrees into thoſe of Reaumur, when higher than 32°, ſubſtract 32, and divide the remainder by 2,23, the quotient is the degree of Reaumur on the *Geneva Thermometers*.—If the degrees of Fahr. be below 32, ſubſtract them from 32 and divide the remainder by 2,23.

To convert the degrees of Reaumur into thoſe of Fahr. if above 0, multiply them into 2,23 and add 32 to the product.—If below 0, multiply them into 2,23 and ſubſtract the product from 32, the remainder expreſſes the degree of Fahrenheit.

Of the Uſes of the foregoing Table.

THE uſe of this table is to diſcover, when different obſervations are made at different times or places and at different degrees of heat, what the real quantity of moiſture is, as pointed out by the hygrometer; for as this inſtrument equally *deſcends* to *dryneſs* when the heat increaſes, or the quantity of vapours in the air decreaſes, and equally *aſcends to moiſture* when the heat *decreaſes*, or the quantity of vapours in the air *increaſes*; it is neceſſary to find on which of theſe cauſes, and to what degree, this variation depends. Now theſe problems may eaſily be reſolved by this table. I ſhall begin by the circumſtances of an *increaſe of heat*.

IF the heat increaſes after the firſt obſervation, and the hygrometer deſcends, three caſes may have happened, 1°. There may have been no *variation of vapour*; or, 2do. There may have been an *increaſe* of vapour but diſproportionate to the heat; or, 3tio. There may have been alſo a *decreaſe* of vapour as well as an increaſe of heat.—We may alſo imagine a fourth caſe, in which there may be an increaſe of vapour proportionate to the increaſe of heat, but in that caſe the hygrometer would not deſcend.

THE firſt ſtep to be taken in this inveſtigation is to obſerve what effect the increaſed degrees of heat ſhould have on the hygrometer, abſtracting

abftracting from the influence of vapour. If the defcent of the hygrometer be fuch as the table fhews to refult from the mere increafe of heat, then there was no increafe or decreafe of vapour; but if the defcent be *fmaller* than mere heat would occafion, then there muft have been an *increafe* of vapour; and if the defcent be *greater*, then there muft have been alfo a *decreafe* of vapour as well as an *increafe* of heat.

Thus, 1°. Suppofe an hygrometer to ftand at XCVIII, at any given degree of heat, and that the heat afterwards increafes 5° of Reaumur, and then that the hygrometer ftands at LXXXIV and four-tenths, and it is required to know to what caufe this afcent is to be attributed.

Remark, in the firft obfervation, at what thermometrical diftance XCVIII ftands from C, that is, what degree of *cold* or decreafe of heat it wants to bring it to C. Now in the fecond column of this table you will find 1°,399 oppofite to, and correfponding with XCVIII, therefore it wants 1°,399 degrees of Reaumur to bring it to C. But if the heat be increafed five degrees more, then it is plain it will want 6,399 degrees of cold to bring it to C. Look then in the fecond column of the table for this number; you will not find it exactly, but you will find it is intermediate between the tabular numbers 6,182 and 6,561, but nearer

to this laſt, conſequently nearly correſponding with the hygrometrical degree LXXXIV, the degree found in the ſecond obſervation; therefore you may conclude, that this degree of *apparent* dryneſs did not proceed from any *decreaſe* of vapours in the air, but ſolely from the *increaſe* of heat.

But, 2do. If in the ſecond obſervation the hygrometer had been at LXXXVI, then it is plain that it ſtands about two hygrometrical degrees higher than five degrees of heat would have brought it to, and conſequently that there was an *increaſe* of as much vapour as would raiſe the hygrometer about two degrees.

3tio. If, in the ſecond obſervation, the hygrometer had been found at LXXX, it is plain that this is four hygrometrical degrees lower than the acceſſion of five degrees of heat would have ſunk it to; therefore there was a decreaſe of as much vapour as would lower the hygrometer four degrees if the firſt temperature had remained.

Again, to diſcover the effects of cold and diſtinguiſh them from the effects of a *real acceſſion of vapour* the ſame caſe will ſerve, only inſerting the obſervations, (for $1^{\circ},399$ degrees of cold would bring XCVIII to C.) Suppoſe then the firſt obſervation to be LXXXIV nearly, and that the air being cooled five degrees of Reaumur,

mur, the hygrometer at the ſecond obſervation ſtands nearly at XCVIII. Oppoſite LXXXIV, I find 6°,561 in the ſecond column, which denotes that 6,561 degrees of cold are required to produce C, but of theſe 6, &c. 5 have actually accrued; therefore only 6,561 — 5 = 1,561 remain; and in the ſecond column I find this number intermediate between 1,961 and 1,399, but nearer to this laſt; therefore the effect produced is ſolely attributable to cold and not to any new increaſe of vapour.—If after the acceſſion of five degrees of cold the hygrometer ſtood at XCIX, then it would follow that as much vapour had been produced as raiſed the hygrometer one degree.—If, on the contrary, on the ſecond obſervation it were found, that, after the acceſſion of five degrees of cold, the hygrometer ſtood at only XC degrees, it would prove that there had been ſuch a *diminution* of vapour as counteracted the influence of cold, and would, at the temperature of the firſt obſervation, make it ſtand eight hygrometrical degrees lower than at firſt.

The uſe of this table with reſpect to obſervations made at different *places*, in different temperatures, and preſenting different hygrometrical degrees, is to find by it in which of thoſe places the real quantity of moiſture is greateſt. This is found by reducing the temperature at one of thoſe places to that of the other, ſo that the effect of mere temperature may be excluded, that thus we may

may fee what hygrometrical degree the obfervation fo reduced would exhibit, whether higher, lower, or equal to the other.

Thus, fuppofing an hygrometer on a mountain, at the temperature of 7° Reaumur, to ftand at XCVI, and an hygrometer on a plain, at the temperature of 15° Reaumur, to ftand at LXXX, and it is required to know at which place moft vapour exifts in the air, let us then reduce the hygrometer on the plain to the temperature of that on the mountain, and fee at what hygrometrical degree, being fo lowered, it would ftand by finding its diftance from C, and to reduce it to 7°, the temperature of the mountain, I muft take away 8° of heat, for 15° — 8° = 7°, and confequently fuperinduce 8° of cold. Now I find by this third table, that an hygrometer at LXXX, let its temperature be what it may, wants but 8°,151 degrees of cold to bring it to C, therefore, by thus fubftracting eight degrees of cold, I find it will arrive very nearly at C, for it wants only 0,151 of it; therefore more vapour exifts on the plain than on the mountain, fince the hygrometer on the mountain at the fame temperature marks only XCVI.

This problem may be refolved *more readily* by infpecting the firft table; for there we find that XCVI, at the temperature of 7° of Reaumur, indicate about 7,6 grains of moifture in the cubic foot, and that LXXX, at 15° Reaumur, indicate 8,045 grains—

or

or ftill more exactly thus: the hygrometer on the mountain being heated eight degrees more (that is to 15°) will ftand at nearly LXXV, for it is already heated to 7°, and by the third table wants 2°.373 degrees of cold to bring it to C; but when heated 8° more, it will require 10,373 degrees of cold to bring it to C, and, confequently, will mark nearly LXXV. Now, by the firft table, LXXV at 15° Reaumur denote 7,205 of moifture, and LXXX at 15° denote 8,045.

Note—I regret much the want of Mr. Lefley's hygrometer (an inftrument as fimple as it is ingenious) to compare its indications with thofe of Sauffure's. See 3d vol. Nicholfon's Jour. 461.